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FUNCTIONAL AND HISTOLOGICAL CHANGES IN THE COCHLEA OF THE GUINEA-PIG RESULTING FROM PROLONGED STIMULATION*[1][2][3]

From the Departments of Physiology and of Otology and Laryngology, Harvard Medical School, the Department of Biology, Harvard University, and Clark University

H. Davis, A. J. Derbyshire, E. H. Kemp,[4]
M. H. Lurie, and M. Upton

Introduction

The possibility has long been recognized of testing various theories of audition by exposing animals to intense tones for long periods of time and then seeking functional or anatomical evidence of specific damage to the auditory mechanism. It is unnecessary to review in detail here the work of Wittmaack, Yoshii, and others, who reported localized zones of degeneration of the organ of Corti so produced in the guinea-pig. Their observations have been cited for years in support of the Helmholtz theory of pitch discrimination. We need only mention that the accuracy of the observations and conclusions has been both vigorously denounced and warmly upheld. Functional tests, in the form of conditioned reflexes, were later employed in an effort to demonstrate partial tone deafness corresponding to the much-disputed local lesions in the cochlea. For the most part these tests have not proved highly conclusive. Discussion has turned upon the adequacy or inadequacy of the various reflexes used in the experiments and the possibility of a "central" deafness. Such central deafness might ensue from extreme over-activity of particular neural mechanisms or might represent merely experimental "extinction" of the conditioned reflex employed for the test of acuity of hear-

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[1]We wish to acknowledge the assistance of Miss Ellen Robinson, of Mr. Paul T. Zahl, and Dr. S. S. Stevens in certain parts of the experimental work.

[2]This research has been assisted directly or indirectly by grants from the Pierce Fund and the Milton Fund of Harvard University, the DeLamar Mobile Research Fund of Harvard Medical School, the Josiah Macy, Jr. Foundation, The American Otological Society, and several anonymous donors.

[3]A preliminary report of these experiments appeared in *Science* (5*a*).

[4]Porter Fellow in Physiology.

ing. A review of this literature by one of us will appear elsewhere (Kemp, 14).

With the analysis of the electrical phenomena of the auditory mechanism (the Wever and Bray effect) into cochlear and neural components (Saul and Davis, 20; Davis, Derbyshire, Lurie, and Saul, 6), it became of fundamental importance to correlate modifications of behavior and specific anatomical lesions in the cochlea (if found) with alterations of the electrical responses of the various sensory and neural structures.

Our experiments will be considered in five groups. The first was a series of cats and guinea-pigs exposed by one of us (M. U.) to a tone of 600 c.p.s. The second series consisted of guinea-pigs exposed in similar fashion to 800 c.p.s. The third series (1933-34), guinea-pigs, were exposed to 600 c.p.s. by E. H. K. at Clark University. They were studied primarily by the method of conditioned reflexes for changes in the auditory limen. The details of this study are presented elsewhere (Kemp, 13), but the results of electrical and histological examination of four of these animals form part of the present report. The fourth series (1934) consisted of guinea-pigs exposed to 2500 c.p.s. by M. U., and the fifth series (1934) by E. H. K. was a repetition, on guinea-pigs, of the original procedures of Yoshii (28) and of Wittmaack (27), which had shown the greatest histological damage of any reported in the literature. The methods of exposure and of electrical test were fundamentally similar for all. The histological preparations were carried out by M. U. for the first two groups and by M. H. L. for the last three.

Methods

Exposure. The first series of animals were exposed to a frequency of 600 c.p.s. at an intensity approximately 85 db above human threshold. The tone, generated by an oscillator and dynamic loud speaker, was maintained continuously 24 hours a day. Twenty guinea-pigs and, at a later time, several cats were placed in a cage 4 ft. square in the center of a large room (30 ft. x 40 ft.). The loud speaker was suspended 24 ins. above the center of the floor of the cage.

The second series consisted of a group of guinea-pigs also exposed by M. U. to a frequency of 800 c.p.s. at an intensity of about 90

db. These animals were kept in a sound-resistant enclosure, 4 ft. x 8 ft. x 4 ft., constructed of celotex. The loud speaker was placed 2 ft. from the floor in one end of the box and the tone was applied 16 hours per day.

The first 2 animals of our third series were guinea-pigs exposed by E. H. K. to a tone of 600 c.p.s. at 65 db for 24 hours a day for 70 days. The other 2 animals were exposed to a similar tone at 95 db for 75 days. The tone was generated by a General Radio 213 audio-oscillator and loud speaker. The wave produced by this oscillator was somewhat damped and harmonics were noticeable. The intensity was determined by matching with standard tones from a Western Electric 2-A audiometer. The animals were confined in a small cage which allowed them to range between 14 and 34 ins. in distance from the loud speaker.

The fourth series, guinea-pigs exposed by M. U. to a tone of 2500 c.p.s. at an intensity of approximately 106 db, were kept in a double-walled box of celotex, whose inside dimensions were 3 ft. 6 ins. x 3 ft. 6 ins. x 3 ft. 6 ins. The loud speaker in this case was supported 24 ins. above the center of the floor on a spring suspension.

The final series of guinea-pigs were exposed by E. H. K. to an Edelmann d_4 whistle blown by a pressure pump. The tone was applied for 15 hours per day. The intensity of this tone was later measured with our calibrated condenser microphone and found to be approximately 97 db above normal human threshold for this frequency. The frequency was determined as approximately 2400 c.p.s. The animals were kept in a small wire cage which kept them within 16 ins. of the whistle.

Electrical Testing. Both nervous action potentials and cochlear response were studied by the apparatus and methods described by Garceau and Davis (8) and by Davis, Derbyshire, Lurie, and Saul (6). The principal alteration made during the period covered by these experiments was the substitution of two stimulating loud speakers (Western Electric, Nos. 555 and 597-A) for the original single instrument. Either of these new loud speakers could be connected to the sound-tube by means of a slide-valve. The No. 555, designed for low tones, was employed up to 3000 c.p.s. The No. 597-A covered the range from 3000 to 10,000 c.p.s. The *Y* arrangement in the sound tube for stimulating both ears simultaneously

was abandoned, and careful attention was given to proper acoustic termination of the tube. The system was nearly free from resonance, as shown by calibration with the condenser microphone. The apparatus was further calibrated by determining audiograms on normal human ears, using the same tubes and speculum as employed for cats.

Minor alterations in the apparatus included the introduction of a dynatron oscillator. This produced stimulating tones of stable frequency and satisfactory wave form down to 60 c.p.s. The recording amplifier was rewired to improve its frequency characteristic above 2000 c.p.s., but it was still necessary to correct for this characteristic in making quantitative comparisons between potentials developed at different frequencies, and in reducing the audiograms to absolute values. The latter correction was necessary since the threshold was taken as the stimulus giving a just-visible wave on the cathode ray oscillograph. This represents a small but finite voltage (*ca.* 1 μv. between cochlea and indifferent electrode), and the strength of stimulus necessary to attain it will vary with the efficiency of amplification of the particular frequency in question. (This limitation applies to the cochlear response, but not to action-potential thresholds, since in the latter case the wave form is essentially the same, irrespective of the frequency of arrival of the nerve impulses.)

We have not attempted an exact evaluation of limens in terms of absolute sound pressures either at the ear drum or at the external auditory meatus. Absolute values may be approximated by means of the condenser microphone, but the attenuation of the sound waves in passing down the short side-tube to the ear is greater than in reaching the microphone through its (shorter) side-tube, and in any case the acoustic impedance and the possible resonant frequencies of the ear differ from those of the microphone and its chamber. Absolute values are probably best estimated by comparison with the limens obtained on normal human ears, and in drawing our conclusions in regard to the experimental animals we rely entirely on deviations from the normal controls of the same species.

Histological. The specimens were prepared by the method of Stacy Guild (2).

Results

Normal Guinea-Pigs. The average sensitivity of the 7 normal guinea-pigs used as a standard of comparison was determined by electrical recording from the promontory or round window. Figure 1 represents the data in the form of an uncorrected threshold curve

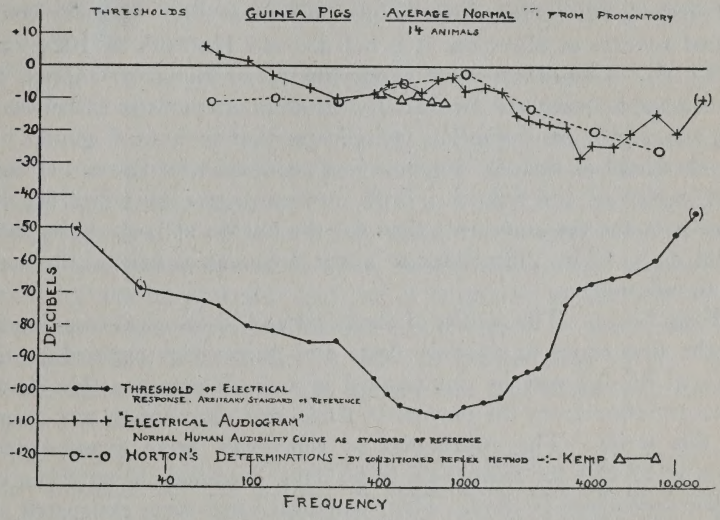

FIGURE 1

AVERAGE ELECTRICAL AUDIOGRAM OF NORMAL GUINEA-PIGS

The threshold is taken as the intensity giving the least detectable wave on cathode ray oscillograph when active electrode is on promontory or round window. Lower solid curve (dots) represents average threshold of audibility expressed as decibels of attenuation of voltage delivered to stimulating loud speaker. Therefore, the greater the sensitivity of the ear, the *lower* is the corresponding curve. No corrections are made in this curve for minor distortions introduced by the sound system. Points in parentheses represent fewer cases and therefore have less reliability than the others. Upper solid curve (crosses), same data expressed as deviations from the average normal human curve determined with the same sound system. "Hearing-loss" is plotted downward as in ordinary audiograms. Therefore, the greater the sensitivity of the ear, the *higher* is the corresponding curve. This relationship holds for all the remaining audiograms in subsequent figures. Broken line (circles), average of Horton's determinations by method of conditioned reflexes of auditory acuity in normal guinea-pigs referred to average normal human as standard, average of 8 animals; (triangles) E. H. K.'s determinations by conditioned reflexes, average of 13 animals, also referred to average normal human. Sigma for E. H. K.'s determinations are 7.7, 7.7, 6.3, 8.8, and 8.2 db, respectively.

and also as an audiogram referred to our normal human standard. The latter is based on 8 ears in the age-group 20 to 30 years. One typical individual was also tested by standard audiometer and found normal. In the same figure are plotted the data for auditory acuity obtained (by E. H. K.) by the method of conditioned reflexes, and likewise the data of Horton (10) by the same method. By the electrical method, our standard deviation is 6.9 db at 1000 c.p.s., by conditioned reflexes at 600 c.p.s. it is 6.3 db, and Horton's at 1024 c.p.s. is 9.1 db. Considering this variability from animal to animal, the agreement between the two totally different methods is rather striking and highly satisfactory. It indicates that in normal guinea-pigs the threshold of cochlear response is a good index of the actual auditory acuity of the animal. Both methods agree in indicating less sensitivity for the guinea-pig than for the human at frequencies above 2500 c.p.s. The differences at lower frequencies may or may not be significant.

First Series. The results of electrical and histological examination of the first series of animals (cats and guinea-pigs exposed to 600 c.p.s. at 85 db) may be summarized in a single sentence: No significant deviation from the normal controls was observed in any animal of this series. The guinea-pigs of this series were exposed (600 c.p.s. at 85 db) for periods up to 60 days, 2 animals being taken out of the cage every 10 days. Their temporal bones were extirpated and prepared for histological examination. The ears of the animals which had been exposed for 10, 24, and 60 days were sectioned and examined, but no disintegration or areas of degeneration comparable to those described by Yoshii were found. Two cats were exposed for periods of 40 and 50 days (600 c.p.s. at 85 db). A final cat, which was most carefully studied electrically, was exposed for 9 months and then kept in the laboratory for 2 months before testing. The tests on these guinea-pigs and cats, made under light avertin anesthesia, included threshold determinations recording from the round window and also from the eighth nerve, and in some cases from the inferior colliculus, the medial geniculate body, and the auditory cortex.[5] The wave form, frequency range, and general character

[5]We shall describe the normal response of the auditory cortex in another communication. Suffice it to say that the "on" and "off" effects are more evident than any characteristic sustained response, and that the limens under our experimental conditions are quite high as compared with those obtained from round window or eighth nerve.

of the responses were indistinguishable from the controls for each region (cf. Saul and Davis, 20). There was no evidence of discontinuity of any function in the neighborhood of 600 c.p.s. On histological examination (M. U.) no cochlear lesions could be recognized.

The only conclusion to be drawn from this series is that cats and guinea-pigs can endure without injury such stimulation as we employed in this series.

Second Series. Five guinea-pigs were exposed by M. U. to a tone of 800 c.p.s. at approximately 95 db for 16 hours a day for 10, 15, 23, 30, and 74 days respectively, and were studied electrically with particular attention to the threshold of response from round window and from eighth nerve. Due to modifications of the stimulating apparatus, it is impractical to make a close comparison of these thresholds with our normal controls, but the tests showed conclusively that there was no circumscribed loss of sensitivity in any part of the frequency range between 200 and 2500 c.p.s. If there was any generalized loss of sensitivity, it was not greater than 15 db. The inferior colliculus of the 74-day animal was also tested with equally negative results.

Third Series. E. H. K.'s guinea-pigs, exposed to 600 c.p.s., had given evidence by the method of conditioned reflexes of a slight impairment of auditory acuity in 5 out of 14 cases (Kemp, 13). The range of frequencies so tested extended only from 400 to 800 c.p.s. The impairment in the positive cases was non-specific over this range. Four satisfactory electrical tests were obtained on his animals under Dial and urethane (Ciba) anesthesia.

Animal III-21, tested December 18, 1933 (exposed for 70 days to 600 c.p.s. at 65 db), was well within normal limits of sensitivity from 112 c.p.s. to 625 c.p.s. From this frequency up to 2000 c.p.s. there was evidence of a slight hearing-loss, averaging 8 db. This was not revealed by the method of conditioned reflexes, and histological examination (M. H. L.) showed a normal cochlea and middle ear.

Animal III-16, December 19, 1933 (exposed 70 days to 600 c.p.s. at 65 db), was tested from 210 to 3000 c.p.s. and was within normal limits except for a slight depression at 3000 c.p.s. The audiogram

showed nothing corresponding to a rather specific loss of sensitivity of 30 db for the conditioned-reflex response to 600 c.p.s. noted in this animal. This latter effect may have been only temporary or may have been central in character. No abnormalities of the cochlea were found on histological examination.

Animal III-51, June 19, 1934 (exposed to 600 c.p.s. at 95 db for 75 days) showed a loss averaging 10 db throughout the range from 15 to 1500 c.p.s. and 12 db in the 400-800 c.p.s. range. At 1750 c.p.s. and above, it was practically normal (Figure 2). The

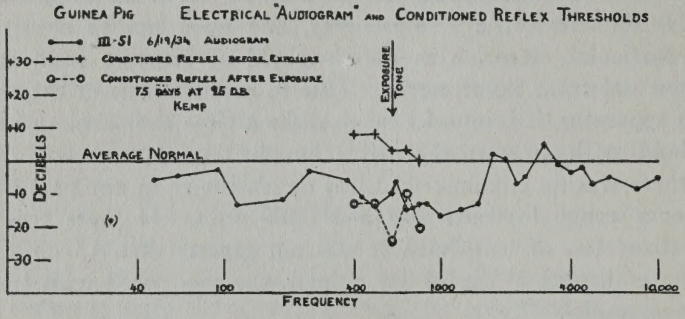

FIGURE 2

ELECTRICAL AUDIOGRAMS OF ANIMAL III-51 AFTER EXPOSURE TO 600 C.P.S. AT 95 DB FOR 75 DAYS

This audiogram is expressed as deviations from the average threshold for normal guinea-pigs determined by the same technique. The thresholds for conditioned-reflex response before and after exposure are also shown. These are referred to our average threshold for normal guinea-pigs by comparing each to normal human hearing.

loss in this case is probably significant, since before exposure this animal had shown definitely *greater* auditory acuity than any other animal of the twelve, about 7 db better than average, and by its reflex responses it showed an average loss of 20 db from its original level in the range between 400 and 800 c.p.s. Histological examination revealed degenerate external hair-cells scattered diffusely through the second, third, and fourth turns. Nowhere were more than approximately one-fourth of the sensory cells affected. Almost invariably it was the innermost hair-cell of the external group which was abnormal.

Another animal, previously exposed to 600 c.p.s. at 95 db for 75 days, died early in the test. Its sensitivity fell to the post-mortem level which we have described elsewhere (6), but showed no evidence of specific impairment at 600 c.p.s. The histological examination revealed no abnormalities.

Chronic otitis media discovered at post-mortem examination explained the very high limens found by the conditioned-reflex method for the fifth animal in this series.

Other electrical tests carried out on animals 51, 16, and 21 included measurement of the maximal voltages generated at the round window in response to strong stimulation. The curves are given in Figure 3. We shall deal elsewhere with the significance of the

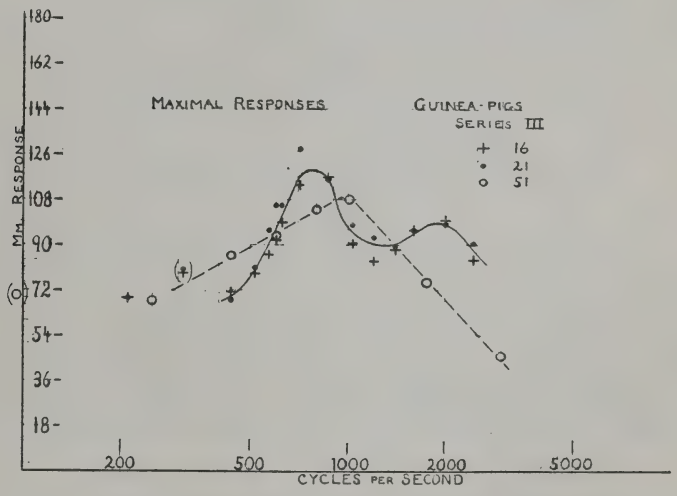

FIGURE 3

MAXIMAL ELECTRICAL RESPONSE FROM THE ROUND WINDOW FOR ANIMALS III-16, -21, AND -51 EXPRESSED AS FUNCTION OF THE FREQUENCY OF THE STIMULATING TONE

The response is expressed as millimeters excursion on the cathode ray oscillograph. The degree of amplification was arbitrarily adjusted to bring the maximal responses to approximately the same size. The significance of the form of these curves will be dealt with elsewhere. They are presented to show the absence of discontinuity at the exposure tone 600 c.p.s. Points in parentheses represent measurements which were confused by the early appearance of higher harmonics.

particular maxima and minima which the curves exhibit. For the present it suffices that they are entirely within normal limits and show no break in the region of the exposure tones. The sound pressures necessary to obtain the maximal responses were also normal, and so were the curves relating magnitude of response to intensity of stimulation (Figure 4). The latter, determined at 500, 600, and

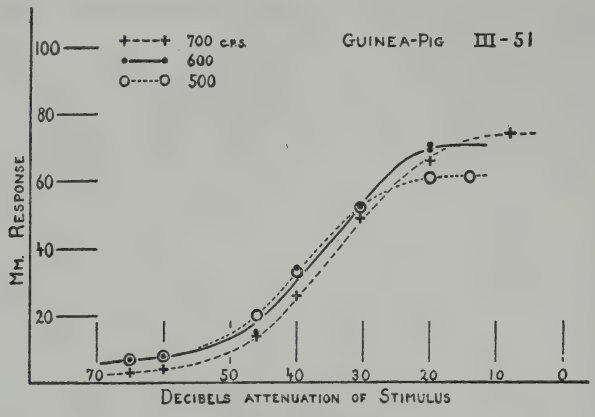

FIGURE 4

SIZE OF RESPONSE IN MILLIMETERS ON CATHODE RAY OSCILLOGRAPH AS FUNCTION OF STRENGTH OF STIMULUS (ANIMAL III-51)

Relative intensity of stimulus is expressed in decibels of attenuation, and therefore on logarithmic scale, with weak stimuli at the left. Zero decibels is the arbitrary intensity resulting from 1 volt delivered to the stimulating loud speaker. These curves at 500, 600, and 700 c.p.s are all within normal limits as to shape, position, and range of variation. In obtaining these data and those of Figure 3, only the fundamental wave is measured. The higher harmonics appearing at high intensities are disregarded.

700 c.p.s., are smooth curves of the typical sigmoid type (cf. Davis, Derbyshire, Lurie, and Saul, 6).

The conclusion from this series is that apparently guinea-pigs can tolerate exposure to 600 c.p.s. at 65 db and at 95 db for many days with at most a slight non-specific loss in acuity.

Fourth series. This series (M. U.'s guinea-pigs exposed to 2500 c.p.s. at 106 db) showed very definite evidences of damage to the sensory mechanism. No conditioned-reflex tests were attempted with these animals, as we had begun to despair of ever producing stimula-

tion deafness. However, complete electrical audiograms and satisfactory histological examinations were obtained in four cases. The audiograms (Figures 5 and 6) are characterized by (1) a more or less generalized loss of acuity in varying degrees, (2) greatest loss

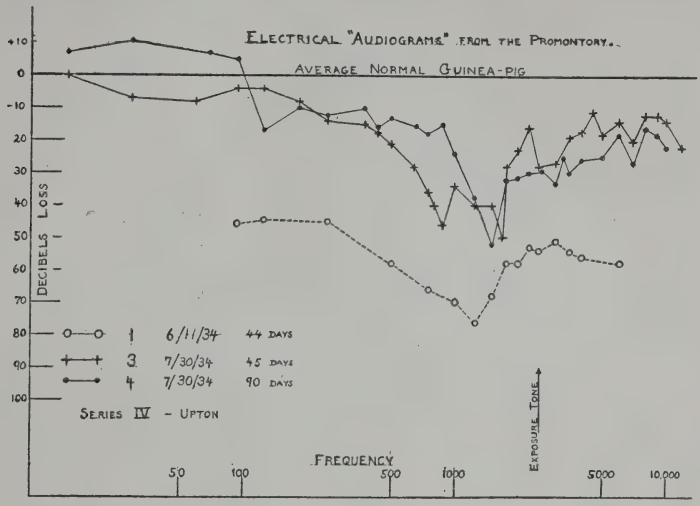

FIGURE 5

ELECTRICAL AUDIOGRAMS REFERRED TO AVERAGE THRESHOLDS FOR NORMAL GUINEA-PIGS (ANIMALS IV-1, -3, AND -4)

Active electrode in contact with the promontory. These animals had been exposed to 2500 c.p.s. at 106 db for 44, 45, and 90 days, respectively. Lowest curve (circles), from animal subsequently found to have intracochlear hemorrhage. Both upper curves (dots) are from animals showing displacement of Reissner's membrane. All three ears also showed degeneration of external hair-cells.

in the range from 1000 to 1500 c.p.s. rather than at the frequency of the exposure tone, and (3) somewhat greater deficiency in the very high range (3000 to 12,000 c.p.s.) than in the very low range (below 300 c.p.s.). The agreement in type of these audiograms is very close. They differ primarily as to degree of abnormality.

Three of these animals presented very unusual features in their responses as well as gross cochlear lesions on histological examination. Animal IV-1 (June 11, 1934) exposed for 44 days, showed the greatest hearing-loss, 77 db at 1250 c.p.s. The action potentials

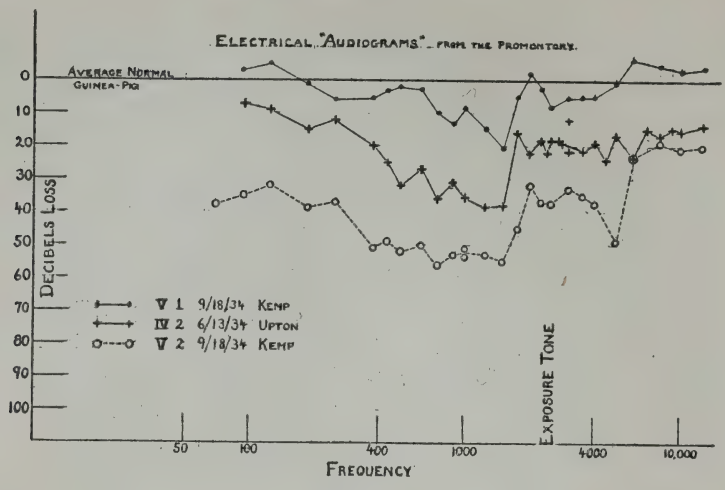

FIGURE 6

ELECTRICAL AUDIOGRAMS REFERRED TO AVERAGE THRESHOLDS FOR NORMAL GUINEA-PIGS (ANIMALS IV-2, V-1, AND V-2)

Active electrode on the promontory. Exposure tone 2400 or 2500 c.p.s at 95 or 105 db. These animals showed no gross intracochlear lesion, but more or less degeneration of sensory cells in the second basal whorl.

obtained from the auditory nerve with coaxial needle electrodes were also studied in this animal. Above 500 c.p.s., the threshold curve coincides almost exactly with the cochlear-response curve, while at lower frequencies it lies below. This indicates that tones below 500 c.p.s. are not registering as effectively as usual and that the audiogram as obtained from the promontory shows 15 db more hearing-loss in this range than actually existed. The wave form of the cochlear response was so distorted that at first we mistook it for pure action potentials. The round window never became electrically positive. The response to a pure tone was approximately a series of negative half-waves, as though the motion of the basilar membrane were abruptly checked at mid-position. The negativity indicates that the membrane could bulge downward into the scala tympani (due to positive pressure on the tympanum) but not upward (cf. Davis, Derbyshire, Lurie, and Saul, 6, p. 317).

During tests on the auditory nerve, a position was found in the auditory tracts lying nearest to the cerebellum which yielded a con-

tinuous spontaneous discharge at the frequency of about 210 per second. The wave form was roughly sinusoidal and the amplitude was not great, but the activity was sharply localized to a region which also gave moderately strong responses to auditory stimulation. We have not encountered this phenomenon in any of the 12 or 15 other normal and pathological guinea-pigs similarly examined.

Subsequent microscopic examination by M. H. L. showed a double lesion. First there was degeneration of the external hair-cells from the last third of the basal whorl extending through the second whorl to the first third of the third whorl, with maximal degeneration in the middle third of the second whorl. Degeneration is shown by the disappearance of the nuclei and outlines of the hair-cells and their replacement by strands of reticular tissue (see Figure 7). The internal hair cells showed no significant damages. The second lesion was a recent unorganized hemorrhage into the scala tympani, the scala vestibuli, and the scala media. In the scala media it rested on the organ of Corti from the basal whorl to the third whorl, with traces in the fourth. The other structures of middle and inner ear were normal. It is not uncommon to find small amounts of blood in the scalae vestibuli and tympani, probably resulting from our experimental procedures, but this is the first case in which we have seen it in the scala media. Its presence explains perfectly the curious distortion of wave form of the cochlear response and its high threshold, but we do not feel sure that it was caused by the exposure to the tone. The opposite ear, not tested electrically, showed no hemorrhage but an identical degeneration of hair-cells.

Animals IV-3 and IV-4 (45 days at 106 db), both tested on July 30, 1934, gave abnormal audiograms closely resembling one another (Figure 5). Both animals showed wave forms of extraordinary complexity in the cochlear response when stimulated strongly at frequencies of about 1000. Even with stimuli near threshold, we commented, while obtaining the audiograms, on the unusually complex waves. Animal IV-4 when stimulated between 900 and 1400 c.p.s. gave back not one tone but a mixture of several, two or more of which were about equal in intensity. Other irregularities can best be described as flutters and squeaks. The details of these responses altered spontaneously from time to time during the experi-

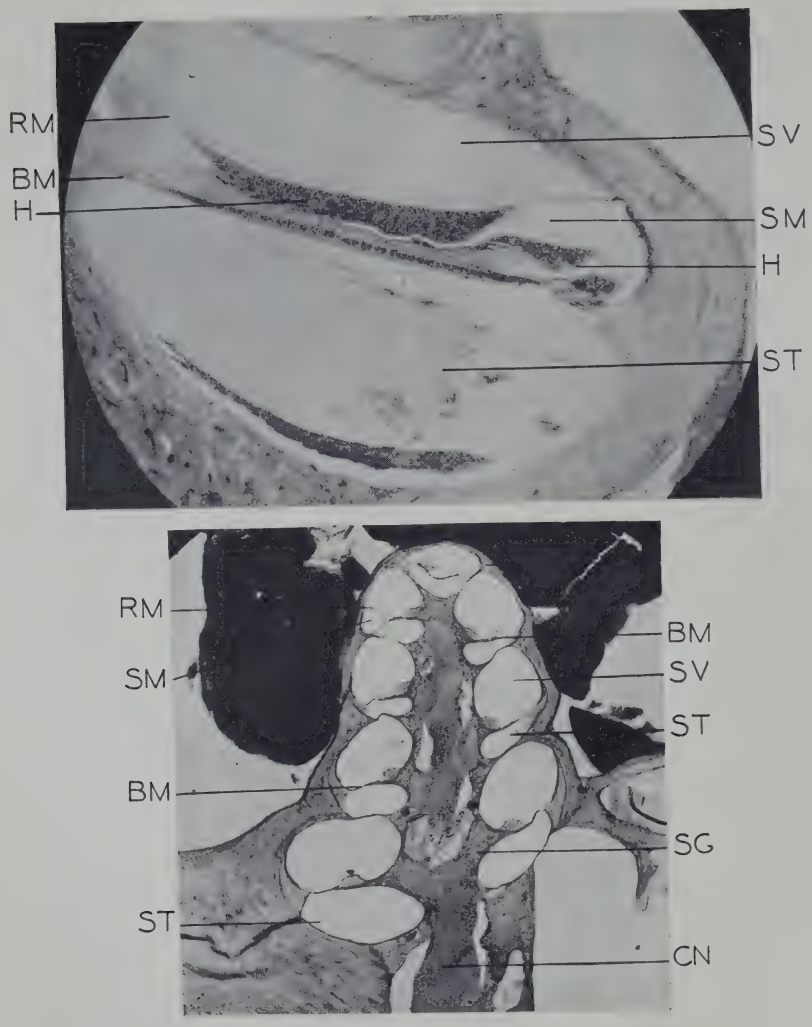

FIGURE 7*A*
FIGURE 7*B*

ments, but the general character remained and was very similar in the two animals. These complexities and distortions are unique in our experience with over three hundred cats and guinea-pigs.

FIGURE 7

SECTIONS OF ANIMALS FROM SERIES IV

BM—basilar membrane
C—Claudius' cells
CN—cochlear nerve
EHC—external hair-cells
EL—external ligament
H—hemorrhage
IHC—internal hair-cells
N—nerve to external hair-cells
P—pillar cells of tunnel
RM—Reissner's membrane
SG—spiral ganglion
SM—scala media
ST—scala tympani
SV—scala vestibuli
TC—tunnel of organ of Corti
TM—tectorial membrane

A. Animal IV-1, basal whorl, x 60, showing hemorrhage in scala media, resting on the organ of Corti and the basilar membrane, and hemorrhage in the scala vestibuli, resting on Reissner's membrane. There is also blood in the scala tympani, resting in the bottom of this channel. This section does not pass through the axis of the cochlea, but is tangential. This explains the position of Reissner's membrane and also the unusual appearance of the organ of Corti. The fracture of the basilar membrane on the right is an artefact of cutting.

B. Animal IV-3, right ear, x 27. Reissner's membrane has been torn and rests on the organ of Corti except in the fourth and the apical whorls. Under high power, the organ of Corti can be seen in the other whorls, compressed under Reissner's membrane.

C. Animal IV-2, right ear, basal whorl, x 400. The organ of Corti is normal except for distortion of the basilar membrane due to cutting.

D. Animal IV-2, second basal whorl, x 400, degeneration of external hair-cells. The nuclei are still present but are abnormal in appearance. This stage does not represent complete degeneration of the external hair-cells. The internal hair-cell is normal.

On histological examination, these animals both showed degeneration of external hair-cells throughout a considerable part of the basilar membrane from the middle of the basal whorl to the third whorl, but much more prominently they showed displacement of Reissner's membrane (Figure 7 *B*). This structure had apparently been torn from the basal to the apical whorl and lay closely over the organ of Corti, compressing and seriously deforming it. This condition, which we have never before encountered, obtained in both of the tested ears and also in the two untested ears from the same animals.

Animal IV-2 (46 days at 106 db; June 13, 1934) showed an audiogram similar in type to the others but with less severe loss of sensitivity (Figure 6). This animal had maintained particularly good health throughout the exposure and had lost no weight. Both ears were tested, but unfortunately the drum of the left ear was

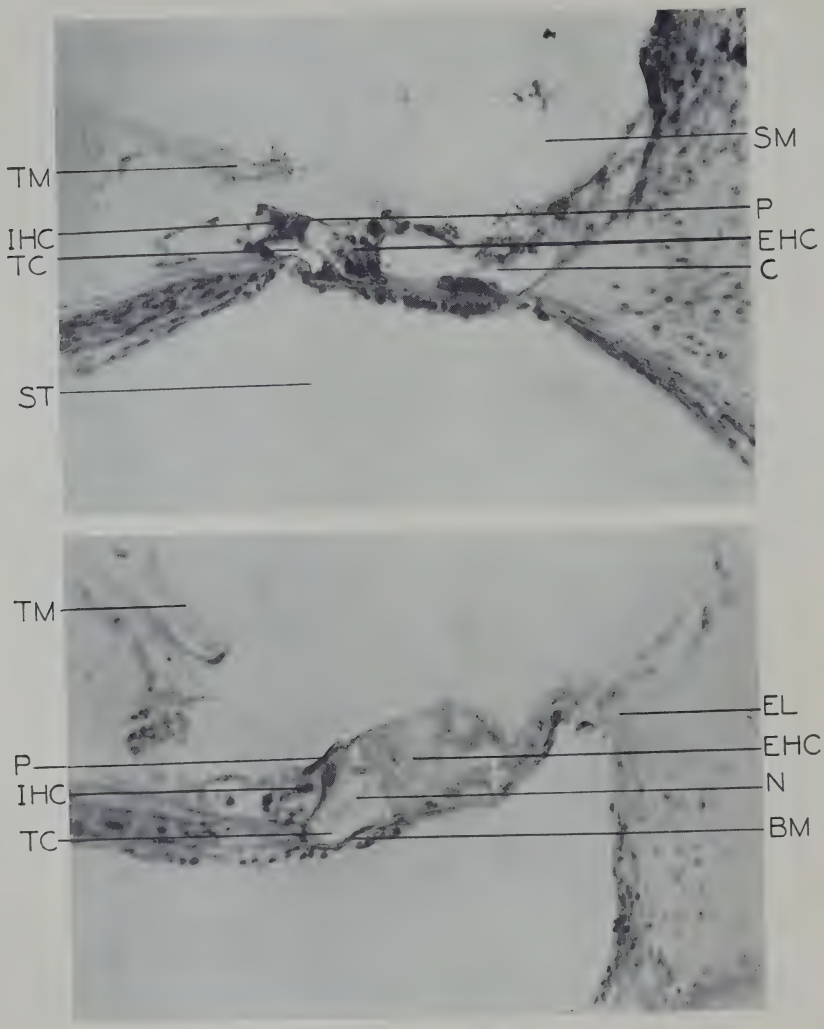

FIGURE 7*C*
FIGURE 7*D*

badly torn during the operation. It is significant, however, that the audiogram of this ear corresponds in form, although with much greater total loss of sensitivity, to that of the ear with intact drum.

This shows that the particular selective loss of sensitivity which characterized these animals does not depend upon modification of the tympanum. The histological findings in this animal were almost identical with those of IV-1 as to the extent and severity of the degeneration. There was no hemorrhage in this animal, however.

Fifth Series. Two of the final series of guinea-pigs were exposed by E. H. K. for 40 days, 15 hours per day, to a d_4 whistle (2400 c.p.c.) at an intensity of approximately 95 db and additional blasts directly into the left ear twice a day during the last 14 days. A third guinea-pig was exposed to daily blasts at 3100 c.p.s. and a fourth to blasts at 4100 c.p.s. These blasts were delivered directly into the auditory meatus. The audiograms of 4 of these animals are given in Figures 6 and 8. They conform very well to the audiograms

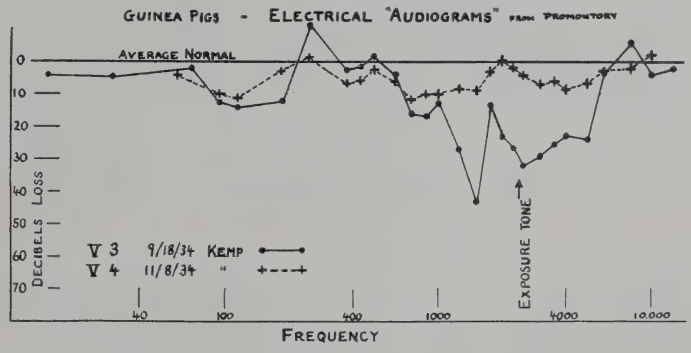

FIGURE 8

ELECTRICAL AUDIOGRAMS REFERRED TO AVERAGE THRESHOLDS FOR NORMAL GUINEA-PIGS (ANIMALS V-3 AND V-4)

Active electrode on promontory or round window. Exposure to 2400 c.p.s at 95 db for 40 days, 15 hours per day. Lower solid curve from animal showing definite degeneration of sensory cells. Upper broken curve from animal kept for 8 weeks after exposure before testing and showing only very questionable degeneration.

of Series IV, and in fact the average curve of this series can scarcely be distinguished from the average for the previous series. Histologically, there were no gross lesions, but all showed degeneration of hair-cells. The severity of the damage correlated well with the loss of sensitivity indicated in the audiograms. V-2 showed degenerate hair-cells from the upper third of the basal whorl through the second

whorl to the beginning of the third. Degeneration was most severe in the middle third of the second whorl. The pathological merged gradually into the normal without sharp boundaries. In this case the degeneration at its worst was more severe than in animal IV-2, since

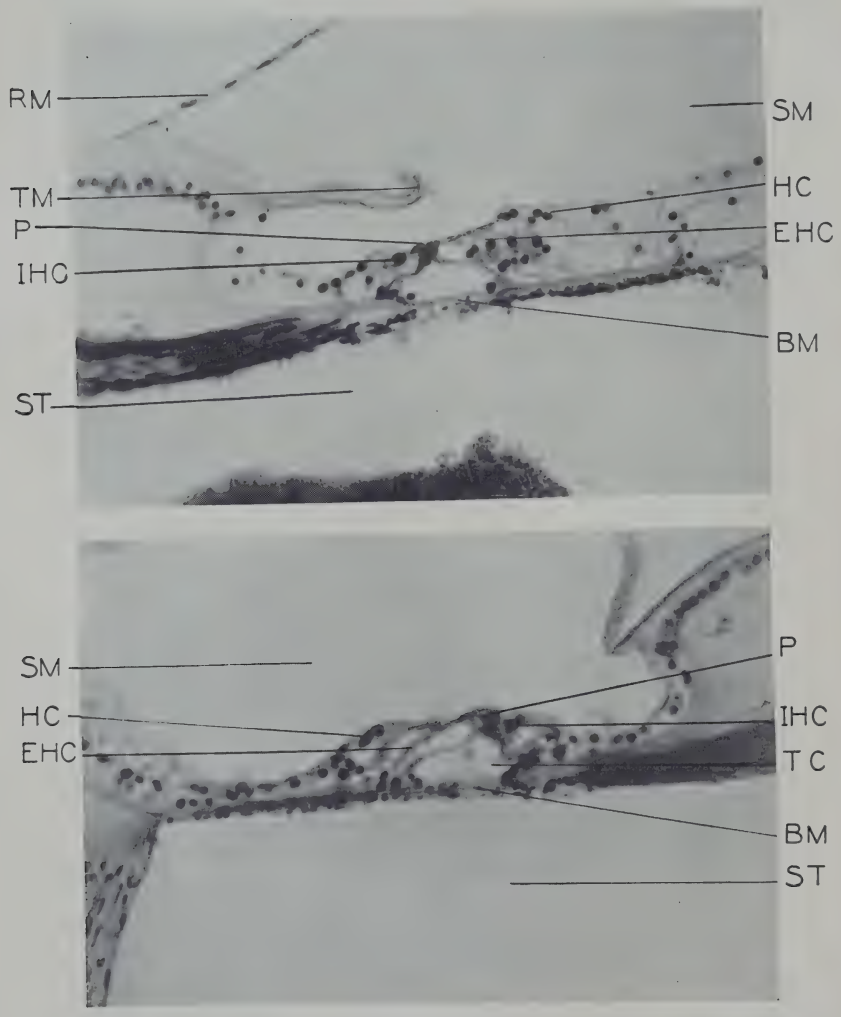

FIGURE 9*A*
FIGURE 9*B*

FIGURE 9

SECTIONS OF ANIMALS FROM SERIES V

BM—basilar membrane
EHC—external hair-cells
HC—Hensen's cells
IHC—internal hair-cells
P—pillar cells of tunnel
RM—Reissner's membrane
SM—scala media
ST—scala tympani
TC—tunnel of organ of Corti
TM—tectorial membrane

A. Animal V-2, left cochlea, basal whorl, x 400. Normal organ of Corti.

B. Animal V-2, second whorl, x 400. This section from the middle third of the second basal whorl shows the maximum degree of degeneration of external hair-cells in our series. No nuclei belonging to these cells can be distinguished. The internal hair-cell appears to be essentially normal.

no definite structure within the hair-cells could be made out (Figure 9). This correlates well with the greater hearing-loss shown by this ear (Figure 6). Toward the edges of the damaged zone, less completely degenerated cells were the prevailing type. The right (untested) ear showed a similar, but even more severe, picture of degeneration.

V-3 (see Figure 8) ranks next in loss of sensitivity and severity of degeneration. The cells resembled those seen in IV-2, but the lesion was more sharply circumscribed. It was confined entirely to the second whorl and was at its worst in the middle third. The audiogram likewise shows a more sharply circumscribed loss, confined to the range from 700 to 5000 c.p.s. It is unusual in the degree of depression evident at the frequency of the exposure tone. The two minima in the audiogram are not reflected in the histological picture, which is that of one continuous zone of degeneration.

V-1 received daily blasts at 3100 c.p.s. at approximately 85 db (see Figure 6). It shows a nearly normal audiogram. Only three points depart more than 10 db from the average normal. They represent a zone of slight impairment extending from 700 to 1700 c.p.s. The temporal bone was given to M. H. L. as "presumably normal," but he found slight but definite degenerative changes in the external hair-cells of the middle third of the second whorl. The changes were practically confined to the innermost cells of the external group.

V-4, whose exposure blasts were at 4100 c.p.s., 70 db (see Figure 8), was essentially normal both functionally and anatomically.

This was a young guinea-pig and had been kept for nearly 8 weeks after exposure before testing. Histologically there appeared a narrow zone of possible abnormality high in the third whorl.

Discussion

Several generalizations may be drawn from the foregoing data. In the first place, there is no doubt but that definite damage may be done to the organ of Corti of guinea-pigs by prolonged exposure to loud tones, as reported by Wittmaack (27), Yoshii (28), Marx (18), Hössli (12), Röhr (19), and others. Not every animal suffers to the same degree from a given exposure, but our observations suggest that an intensity level of 95 db, or thereabouts, is necessary, and, furthermore, that a frequency of 2500 c.p.s. is more effective in causing damage than one of 600 or 800 c.p.s. We are conducting further experiments to verify this conclusion, but it seems especially reasonable if we consider the apparent protective function of the tensor tympani muscle. This muscle is known to contract reflexly in response to sound (Lorente de Nó and Harris, 16). When the tension is applied to the tendon of the tensor tympani or the drum bulged inward by positive pressure in the external canal (Békésy, 1) or negative pressure in the middle ear (Thompson, Howe, and Hughson, 21), the transmission of low tones is reduced more than that of high tones. In collaboration with Dr. S. S. Stevens, we have confirmed this point for guinea-pigs, taking advantage of the fact that many of our animals under Dial-urethane anesthesia exhibit spontaneous rhythmic contraction of the tensor tympani muscle. The latency of the tensor tympani reflex is about 15 milli-seconds (Lorente de Nó, 15). It obviously cannot protect the ear during this time, and this lack of protection at the onset of stimulation may explain the great effectiveness reported by Culler, Finch, and Girden (4) for an intermittent tone in producing stimulation deafness.

Two types of damage may be produced by loud tones. One is a gross damage, such as intracochlear hemorrhage or rupture of Reissner's membrane. While the cases are few enough to allow a reasonable doubt as to whether the damage of this sort, which we found, was directly caused by the stimulation, we believe that the presumption is very strong that this was the case.

The other type of damage is the degeneration of hair-cells similar to that described by previous workers. In our animals it is always the external hair-cells which are involved, and in mild cases it is typically the inner row of these cells which suffers most severely.[6] A plausible inference from this is that it is the inner cells of the external group which are subjected to the most violent mechanical agitation and the internal hair-cells to the least violent. This is in accord with the apparently sheltered position of the internal hair-cells (cf. Figure 9) over the point of attachment of basilar membrane to spiral lamina. In any case, the relative vulnerability of (1) inner cells of external group, (2) the outer cells of external group, and finally (3) the internal group, certainly suggests relative degrees of accessibility to mechanical pressures and movements. The resulting thresholds of stimulation are probably part of the mechanism for discrimination of various degrees of intensity of sound.

The range of frequencies for which the loss of sensitivity is greatest is typically that between 700 and 1700 c.p.s., although the exposure tone was 2500 c.p.s. We have no explanation to offer for this discrepancy, although it may be significant that this most susceptible range is also the range of greatest absolute sensitivity in the guinea-pig. This discrepancy raises an important question as to whether the "audiogram" gives a true indication of the abnormality of audition. Might such animals, when tested by the conditioned-reflex method, show greater loss of sensitivity to the exposure tone than to this lower range? We have begun experiments to investigate this question, but, in the meantime, it seems most reasonable to assume that the "audiogram" is the more trustworthy guide to the loss in function. This question is important from the point of view of auditory theory, since we wish to inquire whether there is a correlation between the frequencies for which the threshold is raised and the position and severity of the anatomical lesions. In the origi-

[6]Our attention has recently been directed to the paper by Guild (8*a*) describing cochlear lesions produced in guinea-pigs by detonations. The milder types of lesion which he describes are almost identical with ours, and he also emphasizes the susceptibility of the external as compared to the internal hair-cells. It is furthermore of interest that his lesions also centered at, or slightly above, the middle of the second whorl. Since his lesions were produced by detonations and not by pure tones, it seems probable that their location was determined by the great sensitivity of this region and the same may well be true of our own.

nal experiments by Yoshii, Wittmaack, and others, it was tacitly assumed that the loss of function would correspond in frequency with the frequency of the exposure tone. This assumption, in spite of its plausibility, is apparently unsafe.

Horton (11), in a recent series of experiments in which he tested sensitivity by the method of conditioned reflexes after 110 hours of exposure of guinea-pigs to a tone of 1024 c.p.s. at 125 db, found that the loss "was no more confined to one tone than to another." He tested the eight octave-tones from 64 to 8192 c.p.s. His individual animals showed great variability of threshold from tone to tone and from test to test, and he does not give the results of histological examination. His observations do, however, show that impairment of sensitivity may extend to tones other than the exposure tone.

In four of our experiments (IV-2, V-1, -2, and -3), the cochlear lesions were sufficiently simple and at the same time definite enough to permit correlation with the audiograms. Three of the audiograms show a maximum impairment at 1250 or 1500 c.p.s. There is also a definite tendency in three of the four cases for the impairment of function to spread toward the lower more than toward the higher frequencies. The anatomical localization of the lesions is not as precise as we might wish, as we have not made complete reconstructions from serial sections, but such precision would probably not be significant in view of the gradual merging of normal to abnormal, which seems to be characteristic. Nevertheless, the four animals in question showed the greatest damage in the middle third of the second whorl. The severity of the degeneration and its extent along the basilar membrane varied from one animal to another, and, if they are placed in a graded series with respect to severity, it is the same series yielded by comparison of the audiograms. The severest case, which showed significant loss of sensitivity for tones below 300 c.p.s., was the only one to show definite abnormality beyond the middle of the third whorl. We believe that this serves to relate the frequency band from 1200 to 1500 c.p.s. with the middle of the second cochlear whorl in the guinea-pig, in the sense that damage in this region results in a loss of sensitivity as determined electrically. This is in accord with the very important work of Crowe, Guild, and Polvogt (2) on human pathological material. They have related abnormalities in the lower half of the

basal whorl with loss of hearing for tones of 2000 c.p.s. and above. From the point of view of a "place" theory, it is satisfying to note that our range of frequency lies at approximately the middle of the audible scale and that the region involved is approximately the middle region of the basilar membrane.

The location of the cochlear hemorrhage and the audiogram associated with it are quite consistent with this correlation. The greatest damage functionally was at 1250 c.p.s., and anatomically the site of the hemorrhage, as well as the greatest severity of degeneration, lay in the middle third of the second whorl. We may regard this case as significant supporting evidence for the conclusion derived from the uncomplicated cases. The cases involving the rupture of Reissner's membrane are also consistent with it.

The excellent correlation which appears in all of these cases between depression of the audiogram and the number of external hair-cells showing signs of degeneration relates these sensory cells very definitely to the so-called cochlear response, which is the basis of these audiograms. No other structures, including the ganglion cells, presented recognizable lesions, while the damage to the external hair-cells was unquestionable and extensive in the severer cases. Coupled with our previous evidence from cats that in a case of congenital absence of the organ of Corti there was no cochlear response whatever (6) and that in a case of complete degeneration of this organ in the basilar whorl there was complete loss of the response for high tones and a progressively diminishing loss for the lower tones (17), it is difficult to resist the conclusion that the sensory cells are specifically responsible for the generation of this response. The establishment of this relationship gives us much greater confidence in drawing conclusions from the cochlear response as to mechanical activities within the cochlea.

The depression of the electrical audiogram and also the anatomical lesions are wide in extent and ill defined as to limit. We must conclude, as indeed we should expect from purely physical consideration of the basilar membrane as a vibrating structure, that its resonance to particular frequencies is rather diffuse. No narrow, sharply limited band vibrates in response to a given frequency, while the remainder rests motionless. On the contrary, a considerable section of the membrane must always be set in motion. Particularly if the

stimulating tone is intense, the spread of activity may be extensive and a loud tone near the middle of the audible range probably activates nearly the entire length of the membrane. The extent of this spread of activity can be better determined by other methods depending on fatigue or masking, as employed on the human ear by Békésy (1), Wegel and Lane (24), and others. We have pointed out elsewhere (6) that the vibration of the auditory mechanism is nearly, although not completely, damped. According to well-known physical principles, such a heavily damped system cannot be expected to show sharp zones of resonance (Wegel and Lane, 24).

All of these conclusions uphold a "place" theory of pitch perception as opposed to a frequency theory, but, except for tones at threshold intensity, we are forced to think of a "zone" rather than a "place" of reception. A given nerve fiber must be capable of activation by a wide range of tones if the tones are sufficiently intense, and the "local sign," on which depends the discrimination of frequency, must often be associated with a large group of nerve fibers and a considerable zone of the sense organ.

Summary

The electrical response of the cochlea (Wever and Bray phenomenon) has been employed as a test of auditory function in guinea-pigs previously exposed to loud tones for long periods of time. The normal threshold by our method lies within 10 db of the human threshold of audibility between 60 and 1500 c.p.s. and about 20 db below it from 2000 to 10,000 c.p.s. It also agrees, within experimental error, from 250 to 10,000 c.p.s. with the threshold of response by the method of conditioned reflexes.

Exposure to frequencies of 600 or 800 c.p.s. for periods up to 75 days at intensities of 60 to 95 db occasionally produced slight widespread functional and histological damage.

Exposure to 2500 c.p.s. at 95 and 105 db for 40 to 45 days regularly produced more or less extensive degeneration of external hair-cells centering in the middle of the second cochlear whorl. The thresholds of electrical response were elevated to a degree and extent corresponding to the severity and extent of the damage. Functional impairment centered at 1200 c.p.s. It is therefore unsafe to assume that stimulation deafness will correspond in pitch to the

exposure tone. In three cases gross lesions of the cochlea were found, correlated with highly abnormal electrical responses.

The results indicate that the electrical response of the cochlea is generated by the hair-cells and also support the "place" or resonance theory of cochlear function.

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LES CHANGEMENTS FONCTIONNELS ET HISTOLOGIQUES DANS LA COCHLÉE DU COCHON D'INDE LESQUELS RÉSULTENT DE LA STIMULATION PROLONGÉE

(Résumé)

La réponse électrique de la cochlée (phénomène Wever et Bray) a été employée comme test de la fonction auditive chez des cochons d'Inde exposés auparavant à des tons retentissants pendant de longues périodes de temps. Le seuil normal selon notre méthode se trouve en 10 db du seuil humain de l'audibilité entre 60 et 1500 c.p.s. et environ 20 db au-dessous de celui-ci de 2000 à 10 000 c.p.s. Il s'accorde aussi, dans l'erreur expérimentale, de 250 à 10 000 c.p.s. avec le seuil de réponse selon la méthode des réflexes conditionnels.

L'exposition aux fréquences de 600 ou de 800 c.p.s. pour des périodes jusqu'à 75 jours à des intensités de 60 à 95 db a produit de temps en temps des dégâts légers fonctionnels et histologiques d'une grande étendue.

L'exposition à 2500 c.p.s. à 95 et à 105 db de 40 à 45 jours a produit régulièrement une dégénération plus ou moins étendue des cellules extérieures des cheveux trouvant son centre au milieu du deuxième verticille cochléaire. Les seuils de la réponse électrique ont été élevés à un degré et à une étendue correspondant à la sévérité et à l'étendue des dégâts. La détérioration fonctionnelle a trouvé son centre à 1200 c.p.s. Il est donc dangereux de supposer que la surdité de stimulation correspondra en hauteur au ton de l'exposition. En trois cas on a trouvé de grandes lésions de la cochlée, corrélées avec des réponses électriques très anormales.

Les résultats indiquent que la réponse électrique de la cochlée est générée par les cellules des cheveux et soutiennent aussi la théorie de "place" ou de résonance de la fonction cochléaire.

DAVIS, DERBYSHIRE, KEMP, LURIE ET UPTON

FUNCTIONELLE UND HISTOLOGISCHE VERANDERUNGEN IN DER SCHNECKE DES MEERSCHWEINCHENS, DIE VON VERLÄNGERTER REIZUNG ENTSTEHEN

(Referat)

Die elektrische Reaktion der Schnecke (Wever und Bray Erscheinung) ist als Kennzeichen der Gehörfunktion bei Meerschweinchen, welche lauten Tönen für längere Zeit ausgesetzt wurden, angewandt worden. Die normale Schwelle bei unserer Methode liegt innerhalb 10 db der menschlichen Schwelle der Hörbarkeit zwischen 60 und 1500 c.p.s. und ungefähr 20 db darunter von 2000 bis 10 000 c.p.s. Dies stimmt auch innerhalb des experimentellen Fehlers von 250 bis 10 000 c.p.s. mit der Schwelle der Reaktion bei der Methode der bedingten Reflexe.

Die Darbietung der Häufigkeiten von 600 bis 800 c.p.s. über Perioden bis auf 75 Tage bei Stärken von 60 bis 95 db verursachen gelegentlich geringen, ausgedehnten, funktionellen und histologischen Schaden.

Die Darbietung von 2500 c.p.s. bei 95 und 105 db für 40 bis 45 Tagen verursachte regelmässig mehr oder minder ausgedehnte Entartung der äusseren Haarzellen, die in der Mitte des zweiten Bogens der Schnecke ruhen. Die Schwellen der elektrischen Reaktion wurden bis zu einem Grad erhoben, der der Heftigkeit und Ausdehnung des Schadens entspricht. Die funktionelle Schädigung ruhte auf 1200 c.p.s. Es wird daher unsicher,

anzunehmen, dass die Reizungstaubheit an Tonhöhe dem Darbietungston entsprechen sollte. In den drei Fällen wurden grobe Verletzungen der Schnecke festgestellt, die mit höchst anormalen elektrischen Reaktionen korreliert waren.

Die Ergebnisse führen zu dem Schluss, dass die elektrische Reaktion der Schnecke durch die Haarzellen erzeugt wird und auch die "Orts-" oder Resonanztheorie der Schneckenfunktion bestätigen.

DAVIS, DERBYSHIRE, KEMP, LURIE, UND UPTON

THE VISIBILITY CURVE OF THE WHITE RAT AS DETERMINED BY THE ELECTRICAL RETINAL RESPONSE TO LIGHTS OF DIFFERENT WAVE-LENGTHS*[1]

From the Psychological Laboratory of Clark University

C. H. Graham and L. A. Riggs

This report is concerned with a description of the visibility curve of the white rat as determined by an electrical recording technique. A primary objective of the research was the determination of the visibility function of the animal for the purpose of utilizing the information in later research where the question of intensity control would be important. In addition we hoped that we might obtain data which would constitute a valuable addition to the information on the functions of the animal itself.

Histological study of the rat's eye (Lashley, 11; Vincent, 13) has so far failed to demonstrate the presence of cones. Furthermore, there is apparently no central region of especially compacted receptors. This peculiarity, together with a possibility for light scattering due to diffusion through a poorly pigmented iris, gives rise, presumably, to the low degree of acuity exhibited by the animal.

The preponderance of behavior data supports the evidence from the histological findings. Thus, Watson and Watson (15) and Munn (12), using the discrimination method, were unable to demonstrate a discrimination on the part of the animal to a wavelength as apart from an intensity difference, a finding which is in accord with an implication of the Duplicity Theory: If a vertebrate is shown to lack cones, then that animal, by the theory, will lack color vision. Contradictory results have been obtained by Walton (14), but Munn's recent criticisms (12) of Walton's method make

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[1]We are indebted to the National Research Council for a Grant-in-Aid to one of us (C. H. G.) which made possible the purchase of the camera used in these experiments. We should also like to express appreciation to Professor Hudson Hoagland of the Department of Physiology, Clark University, for the loan of the string galvanometer over a period of two years.

it obvious that the data are equivocal. The results of the present experiment, as we interpret them, are in harmony with those of Watson and Watson and Munn. In addition we have presented our findings in a form which permits of conclusions with respect to underlying retinal processes.

Apparatus and Method

The method used in these experiments is to obtain in the usual manner string-galvanometer records of the potential changes set up between the front of the animal's eye and some other part of the body upon stimulation of the eye by light. The animal in its holder is placed in an electrically shielded, light-tight box. A microscope objective (part of the optical system) projects through one wall of the box, and the illumination through this impinges on the right eye of the rat. A 250-watt Mazda photographic lamp provides the source of light, and a heat-filter consisting of a 3.7-cm. layer of 1% aqueous solution of $CuCl_2.2H_20$ (Hecht, 7) is placed in the light beam. Condensing lenses concentrate the beam upon an Ilex photographic shutter, the shutter being mounted in sponge rubber to minimize vibration. Directly behind the shutter and in front of the microscope in the dark box is a holder for filters. In general, the optical system is quite similar to one used by Hartline and Graham (6).

Wratten monochromatic filters 71-A, 72, 73, 74, 75, and 76 were used for varying wave-length. Wratten neutral-tint filters of transmissions 0.50, 0.25, 0.1, 0.01 and 0.001, used singly and in combination, gave the required range of intensities. In calibrating the relative energy of the light transmitted through each color filter, a thermopile was inserted in the focus of the beam from the microscope objective at the position occupied during the experiments by the animal's eye. A Leeds and Northrup moving-coil galvanometer was used to measure the current developed in the thermopile. Values obtained for the relative energy of the various color filters are listed in Table 1 together with the central wave-length of each filter.

The energy of filter 76 is given a value of 1.00, and the relative energies of the other filters are listed in relation to 76 as the unit. The central wave-lengths were calculated without regard to the

TABLE 1

CENTRAL WAVE-LENGTH AND RELATIVE ENERGY OF LIGHT TRANSMITTED BY WRATTEN MONOCHROMATIC FILTERS AND COPPER CHLORIDE CELL

Filter number	Central wave-length mμ	Relative energy
71-A	640	1.27
72	610	1.14
73	575	1.14
74	530	1.23
75	490	1.23
76	440	1.00

spectral emission characteristics of the lamp, but the necessary correction would probably affect the values only slightly; the wave-length for any filter is given to the nearest 5 mμ value (Graham and Hartline, 3).

Cotton-thread electrodes make contact with the eye of the animal. These are suspended from U-tubes containing Ringer's solution. Ag-AgCl electrodes dip into the Ringer's solution in the U-tubes, and the electrodes are connected to the input of the first stage of the amplifier system.

The amplifier is similar in principle to one used by Chaffee, Bovie, and Hampson (2). In the first stage is a high-mu tube (UX-240) operating at a grid bias of —1.5 volts. The second stage is a power stage and makes use of a UX-112A tube. An Edelmann string galvanometer constitutes the load in the plate circuit of the second stage, and the plate current is balanced out by means of an opposing current which is taken from a group of batteries in series with the filament battery of the last stage. A variable resistance in the opposing circuit and a voltage divider across part of the voltage supply in the circuit allow for the necessary adjustments in balancing the galvanometer. The resistance coupling between the first and second stages is similar to the type described by Adrian (1). In this type of coupling constant D. C. voltages are balanced out by means of variable potentials from the plate battery supply.

The Edelmann string galvanometer serves to record the retinal potentials. An arc-lamp projects a shadow of the string onto the slit of a moving film camera. A Jacquet timer marks intervals of 0.2

sec. at one side of the film. A small beam of light, deflected from behind the shutter in the optical system, serves to record the duration of the light stimulus.

Preparation. The preparation as used in this study has been found to be convenient for an analysis of visual functions. Retinal potentials may be obtained from the eye of the intact animal, a circumstance which does away with difficulties due to injury from operation, etc. After the rat has become quiet the balance point of the galvanometer may be constant over a period of five or six hours. After the experiment the animal is released and placed in its cage. It may be used again, if necessary, within three or four days.

The procedure used in preparing the rat for the experiment is quite simple. A wad of cotton is inserted in the mouth and adhesive tape is wound about the jaws so as to prevent all motion. The right eye is then protruded gently (Keeler, Sutcliffe, and Chaffee, 9) and the lids held back by adhesive tape. About one-half of the eyeball is then accessible. The animal is taped securely to an adjustable holder and placed in position within the light-tight box, i.e., the right eye is at the focus of the microscope beam. These operations were performed while the animal was lightly anaesthetized. After the animal was placed in the dark-box one electrode was placed on the front of the eye and the other on the muscle layer at the side of the eye.

In a few experiments the indifferent electrode was placed in the mouth. So far as we could see the placement of the indifferent electrode did not influence the form of the curve (Hartline, 5).[2]

Procedure. The preparation was allowed to remain in the dark for two or three hours before each experimental period. By the end of this time the pupil had become widely dilated, and the animal had become quiet. The procedure used in recording the retinal potentials was as follows: The appropriate filters were put in place and the amplifier was balanced to give a zero potential across the galvanometer. The experimenter then opened the camera shutter and exposed the eye to a light stimulus. The expos-

[2]In Experiment 2 (cf. Table 2) we used atropine to immobilize the pupil. So far as we can see, the responses obtained in this experiment are similar to those of the other experiments. Thus, variations in pupil size probably have little effect on the general shape of the retinal potential.

ure lasted for about one second, long enough for the entire *b*-wave to be registered. (In reality the exposure was determined by the appearance of the *b*-deflection. The experimenter watched for this wave and after its appearance closed the shutter. Exposure time thus might vary in some degree; it averaged one second. However, there is no question about anomalous results due to too short exposures. All exposure times are well beyond any possible critical duration.) The preparation was given a strictly adhered-to interval of five minutes for recovery before the procedure was repeated for the next stimulation.

The order of stimulation was determined before experimenting began. Usually a particular color filter was inserted, and a series of exposures with neutral-tint filters of decreasing density was made for that color. After this series another intensity series was run for a second color. In this way the response for each color filter at each intensity was obtained.

A variation of the usual order was made in two cases. In the first, an intensity series was obtained for a single color filter, and then only a single intensity was used for each of the other color filters. The single intensities were of such values that the responses to the different colors were of approximately equal size. In the second case, two color filters were presented alternately in a combined intensity series.

A measured calibrating potential was introduced into the input circuit after every two or three exposures. The resulting deflection of the galvanometer was registered and served to measure the magnitude of the retinal potentials.

Results

The variation in the form of the retinal potential as a function of intensity is well illustrated in Figure 1. In this figure typical retinal responses to white light are presented. Figure 1A represents the response to the highest possible intensity available with the apparatus (i.e., the intensity without any filters in the beam). Figure 1B is a response to an intensity one-hundredth the value of that in 1A, and Figure 1C is a response to an intensity one ten-thousandth that of 1A.

The feature of the response which increases most prominently

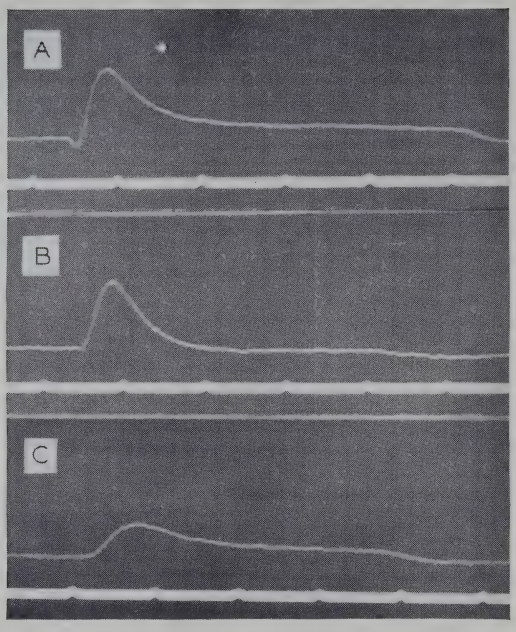

FIGURE 1

RETINAL POTENTIALS TO WHITE LIGHT

Deflection upward indicates increasing positivity of the lead on the cornea. Lower white line gives time in fifths of seconds. Illumination in *B* one-hundredth the value in *A*. Illumination in *C* one ten-thousandth the value in *A*

with an increase in intensity is the *b*-wave. The *b*-wave consists of an abrupt positive deflection which rises above the base line shortly after the onset of the light at low and medium intensities. On reaching its maximum value it declines toward the base line at a rate slower than that of the initial up-swing. After the decline the curve reaches a level which is maintained until the light goes off, and when this occurs there is a complete return to the original zero potential level. It is obvious from Figure 1 that as intensity increases the height and rates of development and decline of the *b*-wave increase. At high intensities the *b*-wave is preceded by a small, rapid negative deflection, the *a*-wave. We have found but slight evidence for a *c*-wave in our records, nor is there

a marked off-effect. The off-effect consists simply of a return to the original base line (Granit, 4). (Our evidence on the absence of a *c*-wave must, of course, be considered as conclusive only for the limits of exposure time which we have used, viz., *ca.* 1 second.)

Figure 2 presents an intensity series obtained with a light of

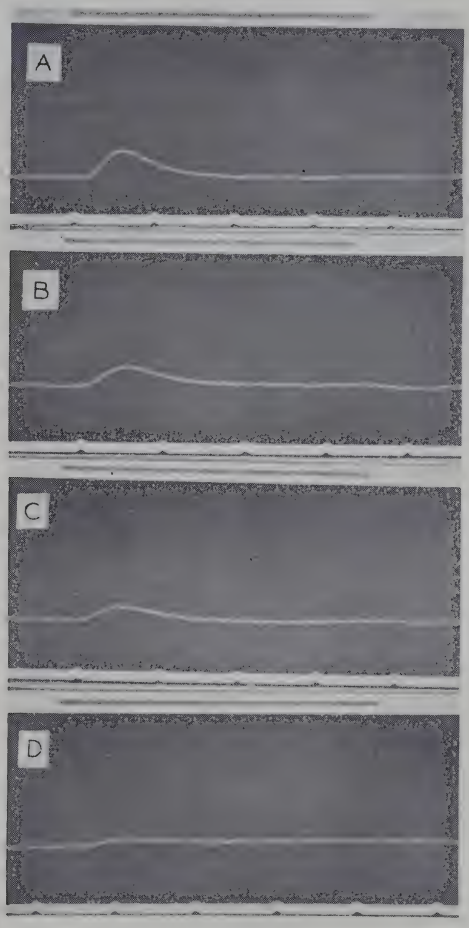

FIGURE 2

RETINAL POTENTIALS TO WAVE-LENGTH 575 mμ

Recording as in Figure 1. Line at top of each record signals onset and duration of illumination. Intensity in *A*: 0.57; in *B*: 0.114; in *C*: 0.057; in *D*: 0:0114. The unit of intensity is: maximum with λ 440 mμ = 1.00

λ575mμ. The figure shows that the intensity effect with this wave-length parallels the intensity effect with white light. In addition, because of the presence of the stimulus-duration line, it shows that latent period decreases with an increase in intensity. The records are from an experiment other than those of Figure 1, and while it is true that the responses, as recorded, are smaller than those of the previous figure, it is obvious that the behavior of the *a*- and *b*-waves and the decline to the steady level are similar to the corresponding features of the records in Figure 1. Indeed, all of our results obtained in ten complete experiments indicate that the retinal response of the rat's eye is one which varies only with respect to brightness. A response for any wave-length may be matched by a response for another wave-length provided the proper intensity value is found for the second wave-length.

This conclusion is supported by the type of finding illustrated in the records of Figure 3. In these records the energies of the stimulating lights have been adjusted until equal, matched responses were obtained. The two upper records (3A and 3B) were obtained with, respectively, λ575mμ at a relative energy value of 1.14 and λ490mμ at a relative energy value of 0.132. An examination of these two records shows that it would be possible (within the limits of the normal fluctuations of the base line) to superimpose the two curves and that all convenient measures of response (e.g., latent period, height of *b*-wave, etc.) would be matched. Clearly, there are no effects evidenced in the retinal potential which can be attributed to the influence of wave-length *per se*.

Figures 3C and 3D consist of responses from the same animal to the same wave-lengths when the energy value of each has been lowered to one-tenth of its value in the upper record. Again, it is obvious that the curves could be superimposed. This finding leads to the conclusion that, at least within the narrow intensity range of one log unit, there is no Purkinje effect; for it is demonstrated that a match made at one intensity level holds for an intensity level one-tenth the value of the first.

Figure 3 demonstrates one other point of importance: If we match two responses with respect to any single measure of response (latent period, *b*-wave, etc.), then these responses will be matched with respect to any other measure. This makes it possible for us

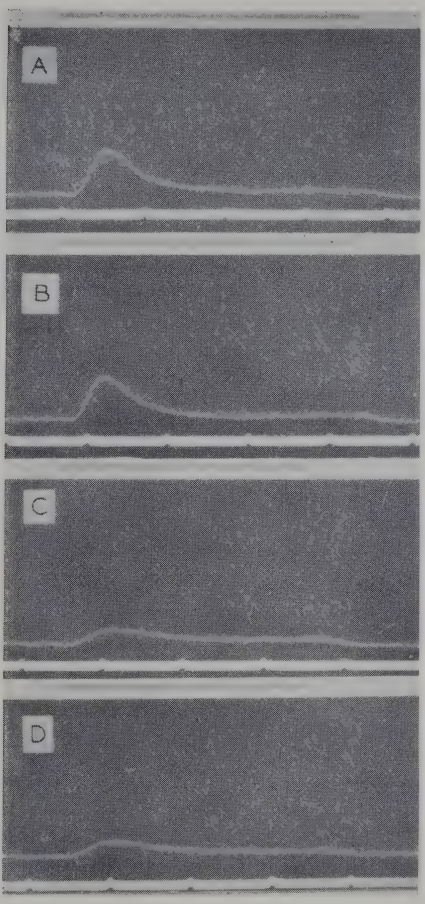

FIGURE 3

MATCHED RESPONSES FOR 575 mμ AND 490 mμ

Recording as in Figure 2. Record *A*: 575 mμ, intensity 1.14; *B*: 490 mμ, intensity 0.132; *C*: 575 mμ, intensity, 0.114; *D*: 490 mμ, intensity 0.0132.

to draw graphs of some measured aspect of the responses as a function of intensity and at the same time to be certain that a constant magnitude of this aspect of response is correlated with a constant magnitude of any other aspect of response throughout the various response-intensity curves for the different wave-lengths. For pur-

poses of analysis we have used the height of the *b*-wave as a measure of response. This is the most obvious feature of the potential change and it can be measured with greater accuracy than, for instance, such an aspect as latent period.

If the voltage at the maximum of the *b*-wave is plotted as a function of log intensity, the curve for each wave-length is similar to those illustrated in Figure 4. In this figure *response*-log *intensity*

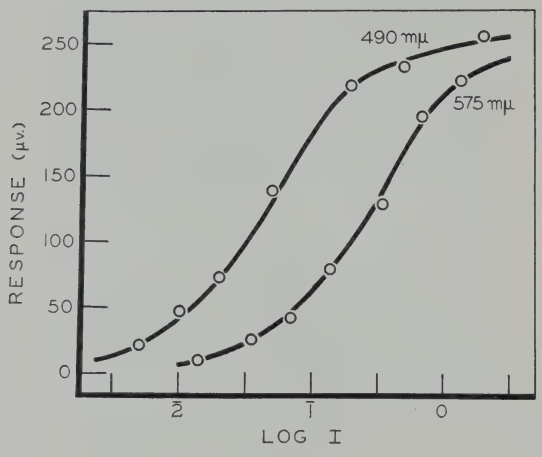

FIGURE 4

RETINAL RESPONSE PLOTTED AS A FUNCTION OF THE LOGARITHM OF THE INTENSITY FOR TWO WAVE-LENGTHS

The lines as drawn are approximately parallel. Explanation in text.

curves are given for λ490mμ and λ575mμ. Extensive determinations were made on these two wave-lengths for the purpose of determining whether or not there is a Purkinje effect (an effect which would appear as a crossing of the curves). Obviously, on analogy with the human-eye data, we should expect a Purkinje shift to appear strongly with these wave-lengths. It is to be noticed, however, that the curves remain parallel throughout their entire ranges. Thus, if we choose some constant value of the ordinate and determine the corresponding abscissa values for the two curves, it is clear that we may move to the right or left from these values by any constant amount (hence by a constant fraction or multiple of the original intensity value) and find the new ordinate values still

constant. This indicates that a response which is matched for a high intensity level will remain matched at a low intensity level. There is no Purkinje effect in the visual response of the white rat as measured by the retinal potential.

This conclusion is borne out more fully in the *response*-log *intensity* graphs for all six color filters used in the experiments. In all cases in the ten complete experiments performed the curves for all wave-lengths are parallel one with the other. Due to conditions of the experiment it was impossible to obtain a great number of points on every curve in any single experiment. However, we always obtained three as a minimum on any curve, and the number determined for any wave-length varied from experiment to experiment. Usually, six or seven points were determined for 490mμ, and five or six for 530 and 575mμ. Fewer (three, four, or five) were determined for the other colors. Careful analysis of the data has convinced us that all the *response*-log *intensity* curves are parallel so far as we can judge. Certainly, they do not cross.

The visibility curve of the white rat may be derived from the various *response*-log *intensity* plots after the manner described by Hecht (7). Visibility is defined as the reciprocal of the relative energy required to produce a constant response. If the intensity values for the curve farthest to the left (that curve corresponding to the most effective part of the spectrum) be given a relative value of one for any magnitude of response, then the abcissa distance through which this curve must be shifted to the right in order for it to coincide with the curve for another wave-length is equivalent to the logarithm of the relative energy required by the second wave-length to produce responses corresponding to those on the left-hand curve. This value is equal to the logarithm of the relative energy necessary to produce a constant response, and it may be obtained for all the wave-lengths used.

Figure 5 presents a graph of the logarithm of the relative energy for the various wave-lengths. The numerical data are presented in Table 2. Data from the last six experiments are plotted; these are the experiments in which technique was most adequate. The points give a good idea of the type of variability encountered from experiment to experiment. The curve as drawn through the points is theoretical, being the curve derived from a consideration of the

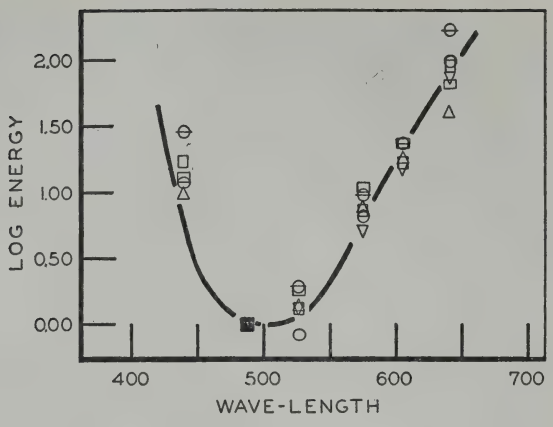

FIGURE 5

THE LOGARITHM OF THE RELATIVE INTENSITY REQUIRED BY VARIOUS STIMULATING WAVE-LENGTHS TO PRODUCE A CONSTANT EFFECT

Explanation in text. The curve is derived from the data on the absorption spectrum of visual purple. In all experiments the energy at 490 mμ is taken as unity: value indicated by solid, black square

mammalian absorption spectrum of visual purple as given by Köttgen and Abelsdorff (10). The absorption value for λ 500 mμ is given a value of unity, and we have plotted the logarithms of the reciprocals of the relative absorption for all the wave-lengths used by these authors. Obviously, our data agree well with the absorption measurements.

The visibility curve may be derived from the curve of Figure 5

TABLE 2

VISIBILITY DATA FOR SIX EXPERIMENTS

	Log energy 640 mμ	Log energy 610 mμ	Log energy 575 mμ	Log energy 530 mμ	Log energy 490 mμ	Log energy 440 mμ
1	2.01	1.37	0.81	—0.06	0.00	1.05
2	1.91	1.19	0.88	0.14	0.00	1.23
3	1.65	1.22	0.92	0.15	0.00	0.98
4	1.85	1.17	0.69	0.14	0.00	——
5	1.80	1.37	1.05	0.22	0.00	1.11
6	2.21	1.39	0.97	0.29	0.00	1.46
Average	1.91	1.29	0.89	0.15	0.00	1.17
Visibility	0.012	0.05	0.13	0.72	1.00	0.07

and the data of Table 2 by taking the reciprocals of the antilogarithms of the log energy values. This curve is given in Figure 6. The points are the averages of the values obtained in the six experiments. The figure shows that the visibility of the white rat is highest in the blue-green with a maximum at about λ 500 mμ. Visibility falls from this value to low values in the red and violet. The curve is derived directly from Köttgen and Abelsdorff's data; the absorption at λ 500 mμ is given a value of one.

Discussion

During this report it has been emphasized that, if retinal responses for any two wave-lengths are matched at a given intensity level, the retinal response will be matched at any intensity level which is a given fraction or multiple of the original intensity level. This has been interpreted as indicating the absence of a Purkinje effect for the response of the rat's eye as measured electrically. If there were a Purkinje effect we should expect that some of the *response*-log *intensity* curves would cross, thus indicating a maximum visibility at high intensities which would be different from that at low intensities. This does not occur; the curves are always parallel.

Obviously the absence of a Purkinje effect in the retinal response is at best merely presumptive evidence against the presence of color vision. Thus, we do not know whether a Purkinje effect as evidenced by discrimination measurements is necessarily paralleled in the behavior of the retinal potential. (It might be conceivable, for instance, that if rods were functional at high intensities they might mask any electrical effects due to cones.) Despite this consideration, however, we feel that the simplest and most direct interpretation of our evidence is to the effect that there is only one type of receptor in the eye of the rat. If we make the conventional assumption that cones are required for color vision we may go further and state that our evidence is opposed to the notion that the rat has color vision. In this respect our interpretation is directly in line with that of Munn (12) and Watson and Watson (15).

The visibility function of the white rat is adequately described by the graph of Figure 6. Visibility is highest in the blue-green

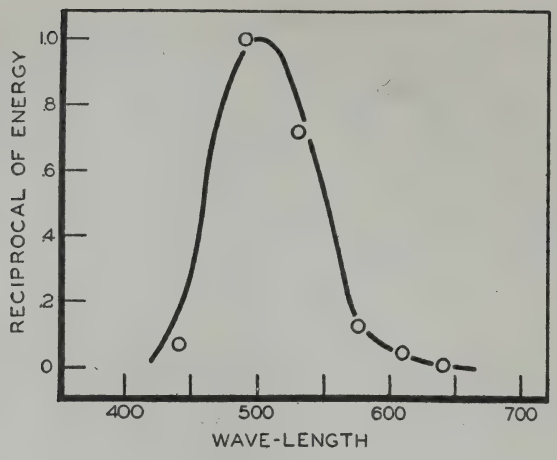

FIGURE 6
THE VISIBILITY CURVE OF THE WHITE RAT
The curve is theoretical. Explanation in text

with a maximum at about λ 500 mμ and falls to low values in the red and violet. It is particularly to be noticed that there is excellent agreement between the visibility values and the data on the absorption of mammalian visual purple (Figures 5 and 6). If we follow the interpretation of Hecht and Williams (8), we may say that the visibility curve of the white rat is representative of the absorption spectrum of the photosensitive substance in the retina, this being, probably, visual purple.

SUMMARY

1. Retinal potentials occurring in response to lights of various wave-lengths and intensities have been recorded from the eye of the white rat by means of a direct-coupled amplifier and string galvanometer.

2. The most obvious feature of the retinal response to light of any of the wave-lengths used is the *b*-wave. This increases with an increase in intensity, while the latent period decreases. At high intensities an *a*-wave appears. There is no definite off-effect.

3. The retinal response of the rat's eye is one which varies

only with respect to brightness: a response for any wave-length may be matched by a response for another wave-length provided the proper intensity value is found for the second wave-length.

4. If responses to different wave-lengths are matched at one intensity level the responses remain matched at another intensity level which is a constant fraction or multiple of the first. As evidenced by electrical recording, there is no Purkinje effect in the eye of the white rat.

5. Visibility is highest in the blue-green with a maximum at about λ 500 mμ and falls to low values in the red and violet. There is good agreement between the visibility values as determined in this experiment and the data on the absorption of mammalian visual purple.

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LA COURBE DE VISIBILITÉ DU RAT BLANC COMME DÉTERMINÉE PAR LA RÉPONSE RÉTINIENNE ÉLECTRIQUE AUX LUMIÈRES DE DIFFÉRENTES LONGUEURS D'ONDE

(Résumé)

1. Des potentiels rétiniens se montrant en réponse à la lumière de différentes longueurs d'onde et intensités ont été enregistrés de l'oeil du rat blanc au moyen d'un amplificateur directement couplé et d'un galvanomètre à ficelle.

2. Le trait le plus évident de la réponse rétinienne à la lumière de n'importe quelle longueur d'onde employée est l'onde *b*. Celle-ci s'accroît avec un accroît de l'intensité, tandis que la période latente décroît. Aux hautes intensités une onde 2 se montre. Il n'y a ni une onde *c* bien marquée ni un effet défini après.

3. La réponse rétinienne de l'oeil du rat est une réponse qui ne varie qu'à l'égard de la brillance: une réponse pour n'importe quelle longueur d'onde peut être égalée par une réponse pour une autre longueur d'onde pourvu qu'on trouve la valeur d'intensité qu'il faut pour la seconde longueur d'onde.

4. Si les réponses aux différentes longueurs d'onde sont égalées à un niveau d'intensité, les réponses restent égalées à un autre niveau d'intensité qui est une fraction constante ou multiple du premier. Comme les enregistrements électriques le montrent, il n'y a pas d'effet Purkinje dans l'oeil du rat blanc.

5. La visibilité est la plus haute dans le bleu-vert avec un maximum à environ 5—m et tombe à des valeurs peu élevées dans le rouge et le violet. Il y a un bon accord entre les valeurs de visibilité comme déterminées dans cette expérience et les données sur l'absorption du pourpre visuel mammifère.

GRAHAM ET RIGGS

DIE BESTIMMUNG DER SICHTBARKEIT DER WEISSEN RATTE DURCH DIE ELEKTRISCHE NETZHAUTREAKTION AUF LICHTER VERSCHIEDENER WELLENLÄNGEN

(Referat)

1. Netzhautpotentiale, die auf Lichtreize der verschiedenen Wellenlängen und Intensitäten folgen, werden vom Auge der weissen Ratte vermittelst eines direkt verbundenen Vergrösserers und eines Fadengalvanometers erhalten.

2. Die bemerkbarste Eigenschaft der Netzhautreaktion auf Licht der verschiedenen Wellenlängen ist die *b*-Welle. Diese nimmt bei einer Zunahme der Intensität zu, während die latente Zeit abnimmt. Bei hohen Intensitäten erscheint eine 2-Welle. Diese ist weder eine gut bezeichnete *c*-Welle noch eine bestimmte Nebenwirkung.

3. Die Netzhautreaktion des Augens der Ratte ist eine solche, welche nur in bezug auf die Helligkeit variiert: eine Reaktion für irgendeine Wellenlänge kann einer Reaktion für eine andere Wellenlänge gleichgesetzt werden, vorausgesetzt dass der geeignete Intensitätswert für die zweite Wellenlänge festgestellt wird.

4. Wenn die Reaktion auf verschiedene Wellenlängen bei einem Intensitätsniveau übereinstimmen, so bleiben die Reaktionen übereinstimmend bei einem anderen Intensitätsniveau, das ein konstantes Bruchstück oder ein Vielfaches des ersten ist. Aus den elektrischen Aufzeichnungen ist ersichtlich, dass es keine Purkinjewirkung im Auge der weissen Ratte gibt.

5. Sichtbarkeit ist am höchsten im Blau-Grün bei einem Maximum auf ungefähr 5—m und sinkt auf niedrige Werte beim Rot und Violett. Es gibt eine gute Übereinstimmung zwischen den Sichtbarkeitswerten in diesem Experiment und den Ergebnissen von der Aufsaugung des Säugetiersehpurpurs.

GRAHAM UND RIGGS

THE EXPERIMENTAL MEASUREMENT OF A SOCIAL HIERARCHY IN *GALLUS DOMESTICUS*: IV. LOSS OF BODY WEIGHT UNDER CONDITIONS OF MILD STARVATION AS A FUNCTION OF SOCIAL DOMINANCE

From the Psychological Laboratories of Clark University

CARL MURCHISON

Three previous papers have appeared in this series (13, 14, 15), and it is highly important for the understanding of this paper that the findings of those three papers be reviewed. A brief abstract of the findings of those three papers is as follows:

> Beginning at sixteen weeks of age, six young roosters are arranged in a hierarchy of dominance, the order being determined by the number of individuals in the group that each rooster is able to defeat in physical combat (Social Reflex No. 2). This order of ranking is revised at intervals of four weeks from the 16th to the 36th weeks. Beginning immediately after being taken from the incubator, these individuals had been tested at frequent intervals in the Social Reflex Runway. This test consisted simply of releasing two individuals simultaneously from opposite ends of the runway, and then observing the time spent and distance traversed by each in running to the other (Social Reflex No. 1). Various operations involving the concepts of physics were applied without great success to these data. Then simple measurements of time and space were applied. When plotted as a function of Social Reflex No. 2, it was found that Social Reflex No. 1, plotted in terms of space alone, was almost truly linear. A theoretical correction of the abcissa units, which agreed with the empirical data, satisfied the requirements of linear function.
>
> Social discrimination in *Gallus domesticus* is identified as it is measured in the Social Discrimination Cage. When the discriminations are plotted against Social Reflex No. 2, a relationship appears which approaches a linear form. When the discriminations are plotted against Social Reflex No. 1, true linearity is approximated at the 36th week. The analyses show that male discriminations for pairs of males are away

from dominance, and that female discriminations for pairs of males are in the direction of dominance. The constancy of this trait in males makes it possible to measure social discrimination in *Gallus domesticus* in units of space.

Social Reflex No. 3 (the sex reflex) is measured in terms of the total treadings in which each individual engages during a period of time. As so measured this reflex in male *Gallus domesticus* is a linear function of Social Reflex No. 1 and Social Reflex No. 2, while in female *Gallus domesticus* it is a function of Social Reflex No. 1. Observations seem to indicate that such subjective matters as sex favoritism, social insults, and social integration may eventually be exhibited as linear functions of such measurable quanta as Social Reflex No. 1, Social Reflex No. 2, and Social Reflex No. 3.

It is pointed out that these or some similar methods, built upon the presupposition that the methods operate on behavior quanta common to the social conduct of all social animals, can reach through the medium of covariable techniques to the eventual formulation of social law.

At this stage in these experiments it becomes desirable to examine some of the physiological relations of these social reflexes. These relations are to be exhibited without prejudice or theory as to causality. It is not entirely necessary that they be examined, as the social reflexes alone can be subjected to an infinite series of analytical experiments without any reference to any physiological basis. It is in the interest of a broader field of investigation that this examination is being made, and not because of any logical compulsion.

Brief History of the Problem of Insensible Body Loss

This problem was recognized as of scientific importance by Sanctorius in 1614 (16), who recorded the variations in his body weight over a period of thirty years. In 1740 Lining (10, 11) weighed himself each day for a year just after arising each morning and just before retiring each night. It was Lining who determined seasonal variations in body weight. Lavoisier in 1790 pointed out the necessity of making a distinction between cutaneous respiration and pulmonary respiration. Colin in 1862 (6) made a series of measurements on himself which indicated a great difference in body loss during repose and during activity. In 1869 von Leyden (17) ap-

plied the method to fever patients, and in 1873 Jürgensen (9) applied the method to normal men as a second variable in the measurement of temperature. Dennig (8) in 1898 studied the relation of water intake to body loss.

The use of an accurate balance scale in connection with a respiration calorimeter initiated the modern era in this field, and was the contribution of Atwater in 1896 (1). Lombard (12) in 1906 perfected an extraordinary balance that would determine losses during a period of a few minutes. Caspari (7) in 1910, while on an expedition, determined daily variations in body loss during variations in activity. In 1910 Benedict and Carpenter (4) made a large number of measurements on subjects at rest and during work. Benedict (2) also applied the method in 1915 to a fasting subject. Benedict and Hendry (5) in 1921 applied the method to a group of girls, and Benedict (3) in 1923 continued the study with two other groups of girls. Benedict and Root (6) in 1926 determined an important relationship between body loss and metabolism, and concluded that accurate determinations of loss in body weight may be used clinically as a method of determining rate of metabolism.

Subjects

The subjects used in this study are the six young roosters described as members of Group D in the previous papers of this series.

Apparatus

The balance scale used in this study is a simple scale that can be purchased for about $40. The entire length of the scale arm represents one ounce, and this arm is divided into one hundred reading units. The individual isolation boxes were 18 inches square, each fitted with a wire mesh floor underneath which a sheet of brown paper could be slipped into place. The Social Reflex Runway was used as the place for the social activity of the entire group.

Procedure

At the end of the 43rd week the six roosters of Group D were carefully weighed and then placed in the six individual isolation boxes. These boxes were placed in a warm dark room. At inter-

vals of four hours each chick was removed from his box, carefully weighed, returned to his box, the feces carefully weighed, and a new sheet of brown paper inserted. The chicks had no food or water during the experimental period, which lasted for 28 hours.

At the end of the 44th week the six roosters of Group D were carefully weighed and placed in the Social Reflex Runway with the five pullets. The runway was in a warm room and was kept brilliantly illuminated during the entire period of 28 hours. During this period none of the roosters had any food or water. The roosters were again carefully weighed at the end of 24 hours and 28 hours. It was assumed that the weight of the feces for each chick during this period would be the same as during the isolation period of the same length of time.

The Raw Data

The two series of measurements are given in all necessary detail in Table 1. The measures of body weight since birth to the 36th week were reported in the first paper in this series (14), while the weights for the 40th week were reported in the second paper (15). The weights at the beginning of the test are in Table 1. The average weight since birth is the average of the monthly averages. It is necessary to assume familiarity with the raw data reported in the three papers already published.

Figures 1-4 contain the record to date of the male dominance

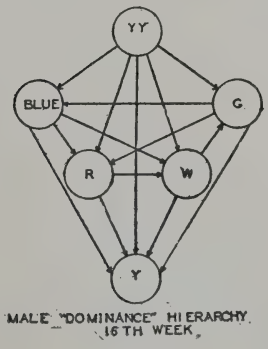


FIGURE 1

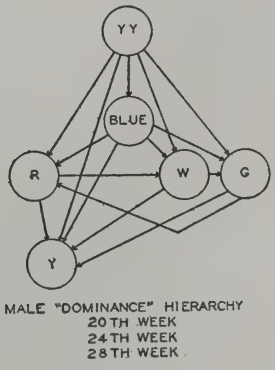
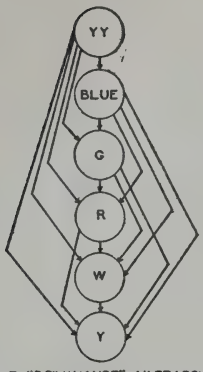


FIGURE 2

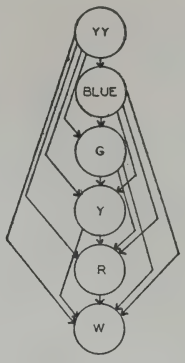
TABLE 1
TEST MADE DURING THE 43RD AND 44TH WEEKS

	YY	Blue	Green	Yellow	Red	White
Weight in kilograms before 24 hours of isolation in darkness	4.00	3.64	3.49	3.92	3.75	3.42
Weight in kilograms after 24 hours of isolation in darkness	3.87	3.56	3.38	3.77	3.65	3.33
Gross loss in grams (*a*)	122.18	81.64	103.19	144.58	98.65	89.02
Weight in kilograms before 24 hours of social activity under light	4.09	3.75	3.59	3.95	3.85	3.40
Weight in kilograms after 24 hours of social activity under light	3.89	3.55	3.42	3.76	3.70	3.26
Gross loss in grams (*a*)	196.16	196.75	169.53	192.78	147.13	131.54
(*a*) and (*b*)	320.34	278.39	272.72	337.36	245.78	220.56
Weight in grams of feces	76.08	55.31	64.07	137.10	68.04	57.26
Net loss in grams during the 48 hours of starvation	244.26	223.08	208.65	200.26	177.74	163.30
Average weight in kilograms since birth	2.65	2.59	2.59	2.68	2.59	2.42
Percentage of average weight since birth lost during 48 hours of starvation	9.21	8.61	8.05	7.47	6.86	6.74



MALE "DOMINANCE" HIERARCHY, 32ND WEEK.

FIGURE 3



MALE "DOMINANCE" HIERARCHY
36 TH WEEK
40 TH WEEK
44 TH WEEK
48 TH WEEK
51 ST WEEK

FIGURE 4

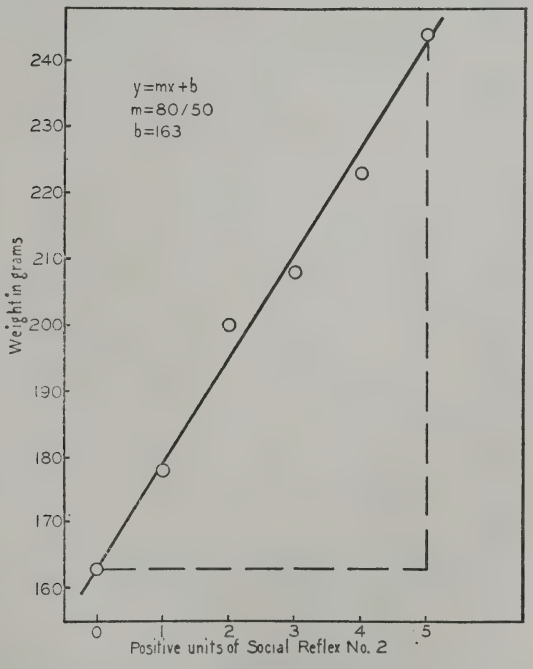


FIGURE 5

LOSS OF WEIGHT IN GRAMS DURING 48 HOURS OF STARVATION AT THE 44TH WEEK AS A FUNCTION OF SOCIAL REFLEX NO. 2 AT THE 36TH WEEK. MALE *Gallus domesticus*, GROUP D.

hierarchy in Group D. The record stops at the 51st week, since "Y" has just died.

Analyses of the Data

1. *In Terms of Net Loss of Weight Without Regard to Percentages.*

a. As a function of Social Reflex No. 2. In Figure 5 the net loss of weight in grams during 48 hours of starvation at the 44th week is plotted as a function of Social Reflex No. 1 at the 36th week. It is obvious that the function approaches linearity.

b. As a function of Social Reflex No. 1. In Figure 6 the net loss of weight in grams during 48 hours of starvation at the 44th week is plotted as a function of Social Reflex No. 1 at the 36th week. The function is almost truly linear.

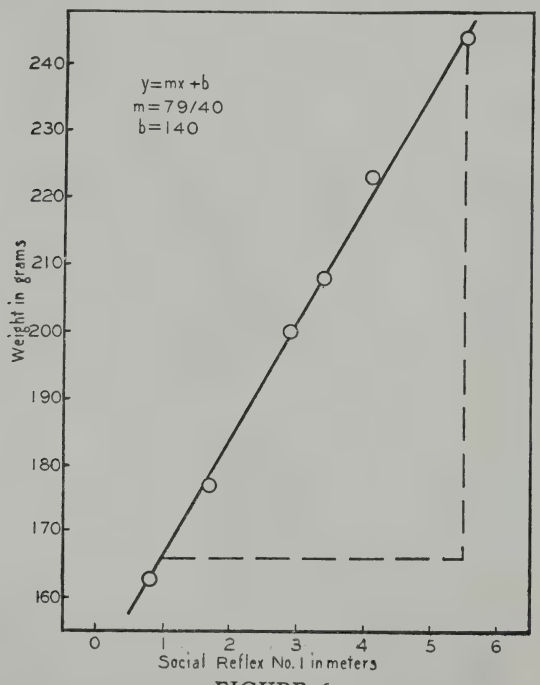


FIGURE 6

Loss of Weight in Grams during 48 Hours of Starvation at the 44th Week as a Function of Social Reflex No. 1 at the 36th Week. Male *Gallus domesticus*, Group D.

c. As a function of Social Reflex No. 3. In Figure 7 the net loss of weight in grams during 48 hours of starvation at the 44th week is plotted as a function of the total observed treadings of Social Reflex No. 3. The function is not linear in the three subjects, but the relation certainly approaches linearity. In Figure 8 the net loss of weight in grams during 48 hours of observation at the 44th week is plotted as a function of the theoretical formulation of Social Reflex No. 3 in number of treadings based on the theoretical assumption that all six of the male members of Group D engage in treading with the total number of treadings remaining constant. In this case the function approaches true linearity.

d. As a function of social discrimination. In Figure 9 the net loss of weight in grams during 48 hours of starvation at the 44th

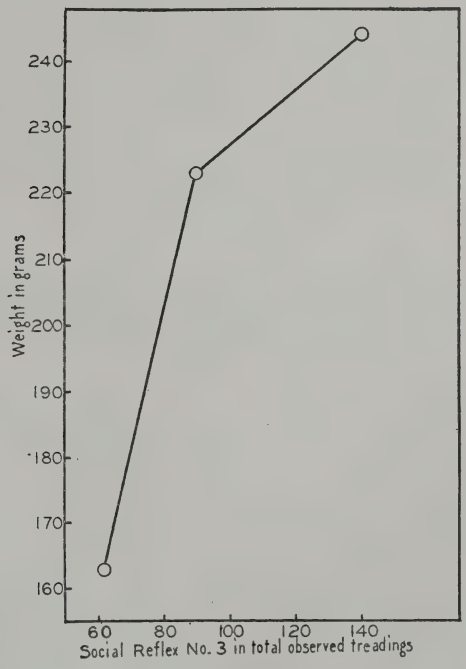


FIGURE 7

LOSS OF WEIGHT IN GRAMS DURING 48 HOURS OF STARVATION AT THE 44TH WEEK AS A FUNCTION OF SOCIAL REFLEX NO. 3 IN TOTAL OBSERVED TREADINGS. MALE *Gallus domesticus*, GROUP D.

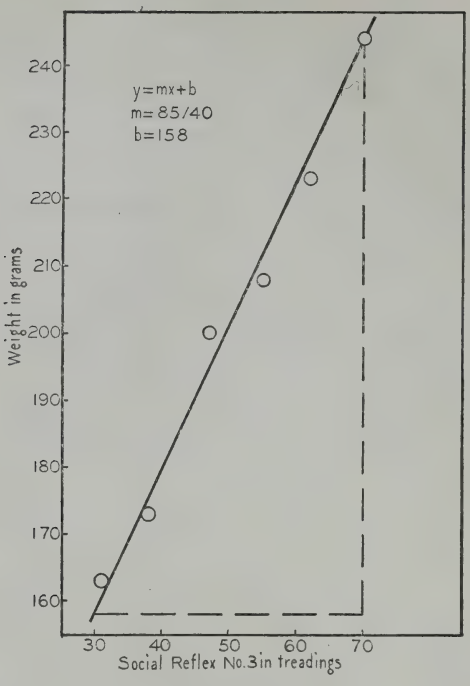


FIGURE 8

LOSS OF WEIGHT IN GRAMS DURING 48 HOURS OF STARVATION AT THE 44TH WEEK AS A FUNCTION OF THE THEORETICAL FORMULATION OF SOCIAL REFLEX NO 3 FOR ALL SIX ROOSTERS. MALE *Gallus domesticus*, GROUP D.

week is plotted as a function of the percentages of times individuals receive negative social discriminations at the 36th week. The function is almost linear.

2. *In Terms of Net Loss of Weight Expressed in Terms of Percentages of the Average Weight Since Birth.*

a. As a function of Social Reflex No. 2. In Figure 10 the net loss of body weight in grams during 48 hours of starvation at the 44th week, expressed in terms of percentages of the average weight since birth, is plotted as a function of Social Reflex No. 2 at the 40th week. With the exception of one experimental point, the function is truly linear.

b. As a function of Social Reflex No. 1. In Figure 11 the

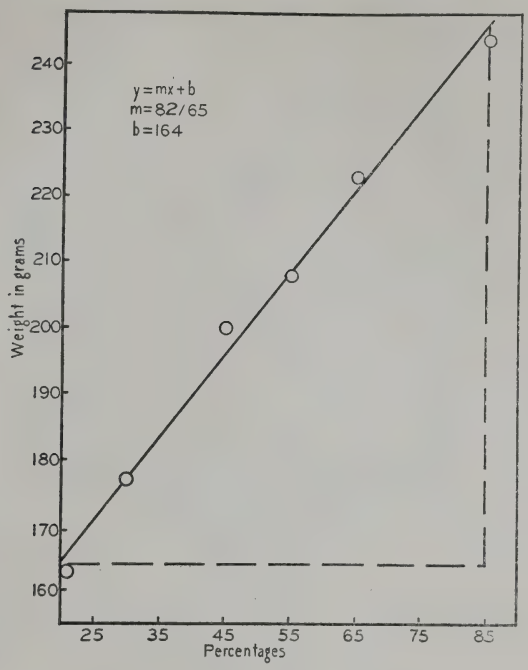


FIGURE 9

Loss of Weight in Grams during 48 Hours of Starvation at the 44th Week as a Function of the Percentages of Times Individuals Receive Negative Social Discriminations at the 36th Week. Male *Gallus domesticus*, Group D.

net loss of body weight in grams during 48 hours of starvation at the 44th week, expressed in terms of percentages of the average weight since birth, is plotted as a function of Social Reflex No. 1 at the 40th week. The function approaches true linearity.

c. As a Function of Social Reflex No. 3. In Figure 12 the net loss of body weight in grams during 48 hours of starvation at the 44th week, expressed in terms of percentages of the average weight since birth, is plotted in relation to Social Reflex No. 3 of the three treading roosters in Group D. The relation is not linear, but indicates the possibility of linearity in a larger hierarchy. In Figure 13 this latter possibility is realized theoretically in all but one experimental point.

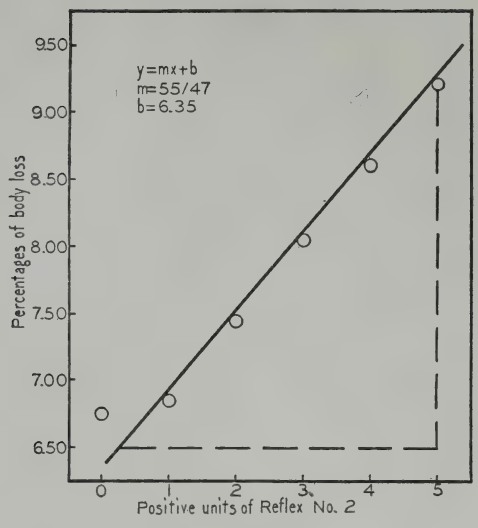


FIGURE 10

NET LOSS IN BODY WEIGHT DURING 48 HOURS OF STARVATION AT THE 44TH WEEK

Expressed in terms of percentages of the average weight since birth, plotted as a function of Social Reflex No. 2 at the 40th Week. Male *Gallus domesticus*, Group D.

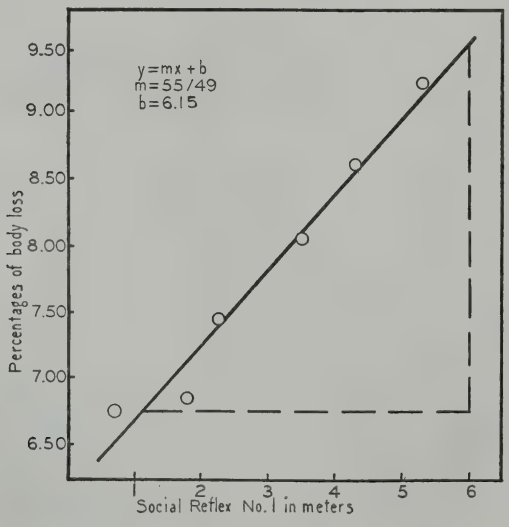


FIGURE 11

NET LOSS IN BODY WEIGHT DURING 48 HOURS OF STARVATION AT THE 44TH WEEK

Expressed in terms of percentages of the average weight since birth, plotted as a function of Social Reflex No. 1 at the 40th week. Male *Gallus domesticus* Group D.

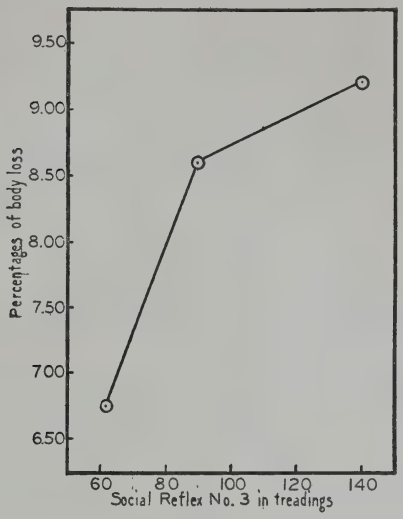


FIGURE 12

NET LOSS IN BODY WEIGHT DURING 48 HOURS OF STARVATION AT THE 44TH WEEK

Expressed in terms of percentages of the average weight since birth, in relation to Social Reflex No. 3 of the three treading roosters in Group D.

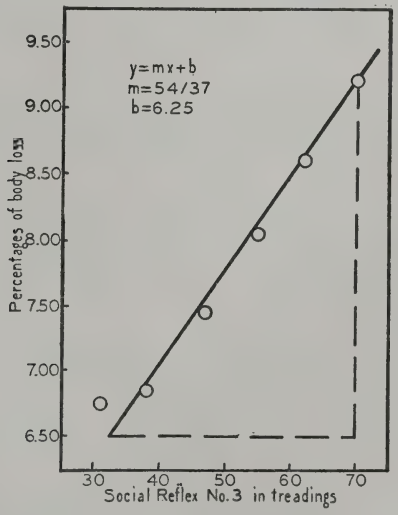


FIGURE 13

NET LOSS IN BODY WEIGHT DURING 48 HOURS OF STARVATION AT THE 44TH WEEK

Expressed in terms of percentages of the average weight since birth, plotted as a function of the theoretical distribution of Social Reflex No. 3 if all six individuals participated. Male *Gallus domesticus*, Group D.

d. As a function of social discrimination. In Figure 14 the net loss of body weight in grams during 48 hours of starvation at the 44th week, expressed in terms of percentages of the average weight since birth, is plotted as a function of the percentages of times each individual receives negative social discriminations at the 36th week. The function is almost truly linear.

Discussion

No assumption is made in this paper concerning the problem of metabolism except such as is given by the reference to the 1926 paper by Benedict and Root (6).

It is not accidental that no reference has been made thus far to the measurement of loss in body weight expressed as a percentage of the body weight just previous to the period of starvation. That has been the traditional method, but there is little theoretical support for such a method. The average weight since birth is a much

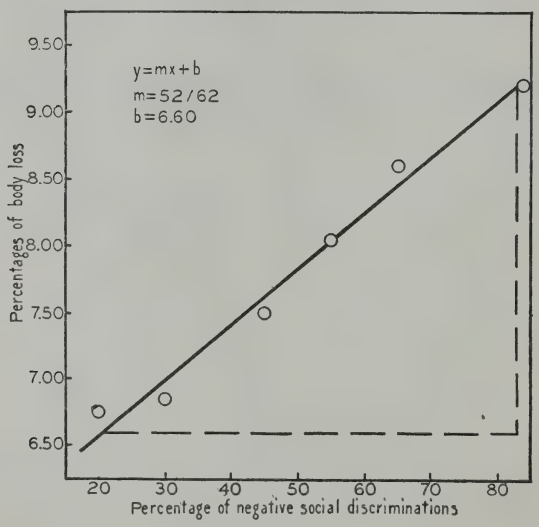


FIGURE 14

Net Loss in Body Weight during 48 Hours of Starvation at the 44th Week

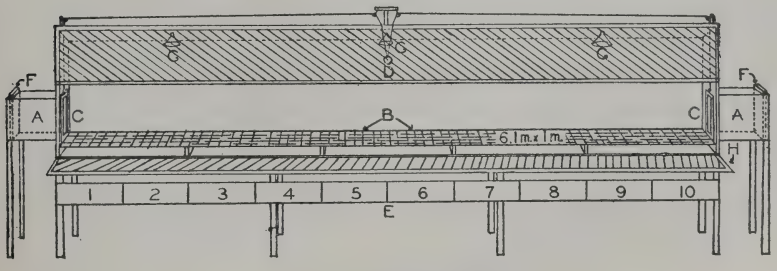
Expressed in terms of percentages of the average weight since birth, plotted as a function of the percentages of times each individual receives negative social discriminations at the 36th week. Male *Gallus domesticus,* Group D.

more stable base from which to determine the percentage of loss. Obviously such a method can be applied only to animals that spend their entire lives in the laboratory.

It is pointed out once more that the raw data concerning the social reflexes and social discriminations have been published in the previous papers of this series, and are not reproduced here (14, 15, 16).

A Further Theoretical Point

It has been pointed out that there is still a slight confusion concerning the reading of the units in the Social Reflex Runway. At the risk of boring those who are conversant with the theory of this method of measurement, the point will be cleared up. In Figure 15 is reproduced once more the runway as it has previously been reproduced in this series of papers. Following the method used by the Bureau of Standards in numbering the units of deflection from the theoretical zero of a balance scale, the theoretical zero of the scale of this runway could not be located at either end. The theoretical zero is located at the exact center of the runway, and the numbers from 1 to 5 are intended merely to indicate the point of origin of the series on the right half of the scale. Figure 16 gives the scale as it is actually used in the readings of experiments. Per-



SOCIAL REFLEX RUNWAY

A - Release Boxes
B - Wire Mesh Floor
C - Release Doors
D - Release Ring
E - Scale (1 unit = .61m)
F - Doors
G - Electric Lights
H - Hinged Door

FIGURE 15

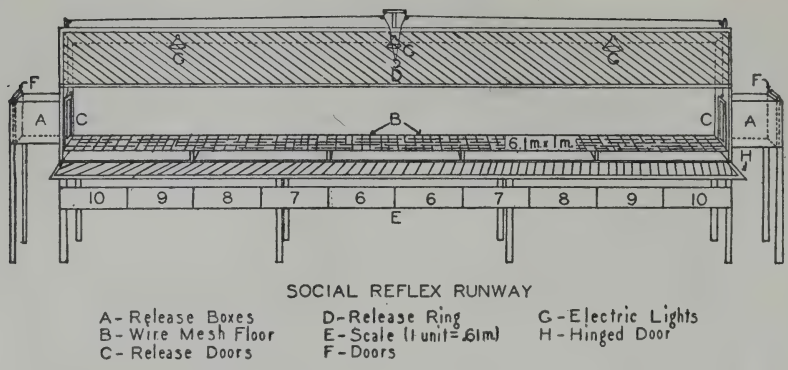


FIGURE 16

haps this improper method of printing the scale will clear up any confusion concerning how readings are taken from such a scale.

General Summary

For male *Gallus domesticus* under the conditions of this experiment the net loss in body weight, over a divided period of 48 hours of starvation containing equal increments of inactivity and social activity, approximates true linear functions of Social Reflex No. 1, Social Reflex No. 2, Social Reflex No. 3, and of social discrimination. The linearity of these functions remains highly constant whether the net loss in body weight is expressed without reference to percentages or is expressed in terms of percentages of the average weight since birth. These findings support the validity of the identification and measurement of the three social reflexes, but raise no question as yet concerning the relation of the reflexes to the general biochemical problem of metabolism.

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LA MESURE EXPÉRIMENTALE D'UNE HIÉRARCHIE SOCIALE CHEZ *GALLUS DOMESTICUS:* IV. LA PERTE DU POIDS DU CORPS DANS LES CONDITIONS D'UNE INANITION LÉGÈRE COMME FONCTION DE LA DOMINANCE SOCIALE

(Résumé)

Pour *Gallus domesticus* mâle dans les conditions de cette expérience la perte nette du poids du corps, dans une période divisée de 48 heures de l'inanition contenant des accroissements égaux de l'inactivité et de l'activité sociale, approche des vraies fonctions linéaires du Réflexe Social No. 1, du Réflexe Social No. 2, du Réflexe Social No. 3, et de la discrimination sociale. Le caractère linéaire de ces fonctions reste très constant soit que l'on exprime la perte nette du poids du corps sans égard des pourcentages, soit qu'on l'exprime en termes des pourcentages du poids moyen depuis la naissance. Ces résultats soutiennent la validité de l'identification et de la mesure des trois réflexes sociaux, mais ne causent aucune question jusqu'ici sur la relation de ces réflexes au problème général biochimique du métabolisme.

MURCHISON

DIE EXPERIMENTELLE MESSUNG EINER SOZIALEN HIERARCHIE BEI *GALLUS DOMESTICUS*: IV. DER VERLUST DES KÖRPERGEWICHTS UNTER UMSTÄNDEN DER MILDEN VERHUNGERUNG ALS EINE FUNKTION DER SOZIALEN HERRSCHUNG

(Referat)

Bei dem männlichen *Gallus domesticus* unter den Umständen dieses Experiments nährt sich der über einen geteitlen Zeitraum von 48 Stunden der Futterentziehung erstreckende reine Verlust des Körpergewichts, der gleiche Vermehrungen von Untätigkeit und sozialer Tätigkeit enthält, den wahren Linearfunktionen des sozialen Reflexes Nr. 1, des sozialen Reflexes Nr. 2, des sozialen Reflexes Nr. 3, und der sozialen Unterscheidung. Die Linienförmigkeit dieser Funktionen wird ohne Hinweisung auf Prozentsätze oder in bezug auf Prozentsätze des Durchschnittsgewichts seit der Geburt ausgedrückt. Diese Befunde bestätigen die Gültigkeit der Identifizierung und Messung der drei sozialen Reflexe, aber erheben keinen Zweifel bis jetzt über das Verhältnis dieser Reflexe zum allgemeinen biochemischen Problem des Metabolismus.

MURCHISON

A DISCRIMINATION BASED UPON A CHANGE IN THE PROPERTIES OF A STIMULUS*

From the Biological Laboratories of Harvard University

B. F. SKINNER[1]

I

Let two stimuli, $S_{AB..}$ and $S_{AB..L..}$, possessing the same properties except for the property L, elicit a conditioned response R. Then a discrimination may be established by extinguishing the reflex ($S_{AB..}$—R) while continuing to reinforce the reflex ($S_{AB..L}$—R), or *vice versa*. This formulation has been tested experimentally by showing that a curve for the establishment of a discrimination has the principal properties of a curve for extinction (1), but other implications remain to be examined. The problem primarily concerns induction—the mutual influence of two reflexes possessing properties in common—by virtue of which a secondary change in the state of one reflex (such as extinction) cannot take place without affecting the state of the other. The extent of this influence is a function of the degree of community of properties and is therefore inversely a measure of the significance of a differentiating property in defining a stimulus class (cf. 2). In the establishment of a discrimination the inductive influence is attenuated (1).

In this formulation L may be a *stimulus* (having the dimensions of energy), in which case it is added to $S_{AB..}$ as a component member; or it may be a single *property* (a given wave-length or intensity, for example, where S_{AB} is light), in which case it is not capable of independent existence as a stimulus (3). The basis for the discrimination may also be a *duration* or a *change* from one property to another, but in this case a more involved formulation is required. We cannot write "change" and "duration" directly for L on equal terms with "intensity" or "component member" (even after specifying what the change is from and to or the duration measured from), be-

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cause change and duration involve temporal sequences. If we wish to establish a discrimination upon the basis of a change from $S_{AB..}$ to $S_{AB..L..}$, we must show that it is not based upon L alone—that is, that the discrimination is not between $S_{AB..}$ and $S_{AB..L..}$ irrespective of the act of changing from one to the other. In order to do this we must show that $S_{AB..L..}$ is effective immediately after a change from S_{AB} but not otherwise. In other words, we must show a distinction between $S_{AB..L..}$ and $S_{AB..\lambda \rightarrow L..}$ (where λ = absence of L, or some other value where L is a property); and in establishing the desired discrimination we must break down the induction between them. We formulate a "change" by distinguishing between $S_{AB..L..}$ and $S_{AB..\lambda \rightarrow L..}$. The latter expression contains a time factor as required, which need not be more rigorously defined for our present purposes. The case of "duration" is similar but will not be treated in this paper.

II

We begin by breaking down the induction between $S_{AB..L..}$ and $S_{AB..}$, and *vice versa,* in successive discriminations. One set of conditions is maintained until a discrimination is well established and then exactly reversed. We may turn directly to the behavior of the organism under this change.

In the discrimination previously studied (1, 5, 6) a white rat came to respond to a lever by pressing it downward whenever a light was present (when the response was reinforced with food) but not to respond in the absence of the light (when the response was not reinforced). We describe this as the extinction of ($S_{AB..}$—R) and the concurrent reinforcement of ($S_{AB..L..}$—R). If we reverse this discrimination, the responses in the absence of the light are reinforced and those in the presence of the light are not. The behavior of the rat during the change may be studied with the method of periodic reconditioning already described, for details of which the reader is referred to an earlier paper (1). The procedure for a single day may be outlined as follows. Originally the light is on at the release of the rat and the first response is reinforced. The light and the food-magazine are then turned off, and the succeeding responses to $S_{AB..}$ are not reinforced. When the discrimination has become well advanced, very few responses occur in the absence of the light. At the end of the interval (five, or in the first of the fol-

lowing experiments six, minutes) both light and magazine are turned on. The rat responds after a latency of the order of five or six seconds (see below), and the response is reinforced. The light and magazine are then turned off for another interval and this is repeated for one hour, at the end of which the rat is removed from the apparatus until the following experimental day. *On the day of reversal* the first response is in the dark and is reinforced; the light is then turned on for five (or six) minutes and no response is reinforced. The light is then turned off, the next response reinforced, and the light turned on again; and this procedure is repeated for one hour. All responses are recorded automatically in the form of number-*vs.*-time graphs.

From information already available we can predict the principal aspects of the behavior of the rat on the day of reversal. We know that after the development of a discrimination an extinction curve displaying certain characteristic properties will be obtained if the previously reinforced stimulus ($S_{AB..L..}$) is presented continuously without reinforcement (5). If we disregard the short periods of $S_{AB..}$ (no light) which occur every five or six minutes we should expect an extinction curve at the beginning of the day of reversal. Such a curve need not have so great an area as a curve for original extinction (5). We also know that periodic reinforcement of the previously extinguished member ($S_{AB..}$) will produce a return to the original strength under periodic reconditioning (5). Third, we know that curves of these two sorts sum algebraically when the conditions for their development exist simultaneously. This was reported without data in (1, footnote, p. 324). A typical curve is shown in Figure 1. It was obtained when a rat in which the response to the lever had been well conditioned was transferred directly to the procedure of periodic conditioning without previous extinction. The first part of the curve is for the extinction of the reflex and shows what we may interpret, on the basis of previous experiments, as a cyclic deviation within a logarithmic envelop, which has been indicated with a dotted line. During the development of this curve, the reflex is being reconditioned at intervals of five minutes, and a curve of positive acceleration approaching a constant high slope should appear (1). The experimental record shows the emergence of this curve, which cuts across the curve for extinction and sums

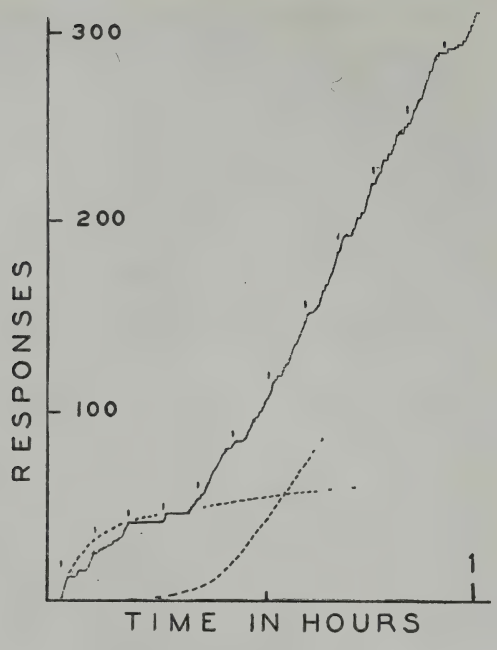

FIGURE 1

with it. The inferred course of the second curve is indicated with a dotted line to demonstrate the summation. The dotted curves are not offered as a quantitative analysis.

We should expect a compound curve of this sort on the day of reversal, but with one modification. The curve of positive acceleration should approach, not a constant slope, but the curve of negative acceleration along which the new discrimination is to develop. In Figure 1 no discrimination is possible, since it for simple periodic reconditioning. In the present case a discrimination is expected. The resulting curve should therefore be composed of at least three separate parts, and in spite of its complexity it is actually obtained experimentally.

A group of eight male rats, approximately 110 days old at the beginning of the experiment, were put through the procedure for discrimination with an interval of reconditioning of six minutes. On the eighth day of the discrimination the conditions were reversed.

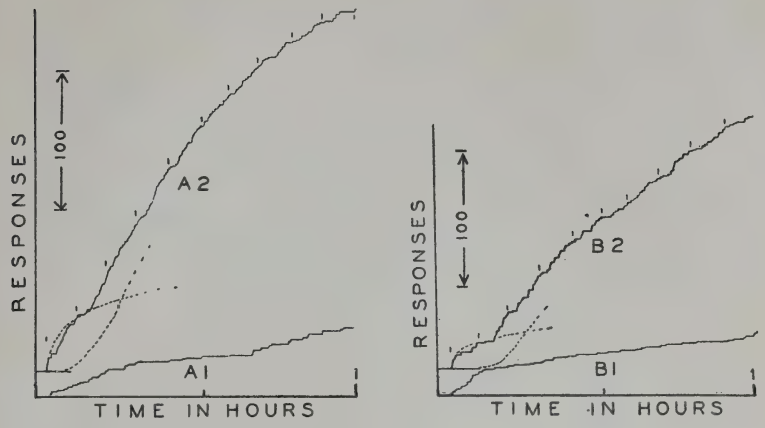

FIGURE 2

Two records for the day of reversal are given in Figure 2. The records *A*1 and *B*1 are for the seventh day of the discrimination. Those marked *A*2 and *B*2 are for the day of reversal, and show the predicted characteristics. They begin with extinction curves, but curves of positive acceleration cut through these and lead to negative accelerations as the new discriminations develop. These are typical records. The result may be less clear when the rate fails to return to its full value under periodic reconditioning (two out of eight cases in the present experiment) but from the nature of these exceptions (see below) they should not affect the present argument.

Four typical records for the whole process are reproduced in Figure 3. They are obtained by plotting the end points of the daily records and copying the intervening records free-hand. The first three days in the figure are for periodic reconditioning at six-minute intervals. On the fourth day (at the vertical broken line) the discriminatory procedure was begun, and the next seven days show the development of the discrimination. The minor deviations in these records resemble those previously reported (1). The daily records *A*1 and *B*1 in Figure 2 are shown by brackets in the present figure. At the second broken line the conditions were reversed. The detailed records *A*2 and *B*2 in Figure 2 are also bracketed. In the other two curves the return to the periodic slope is slower and never

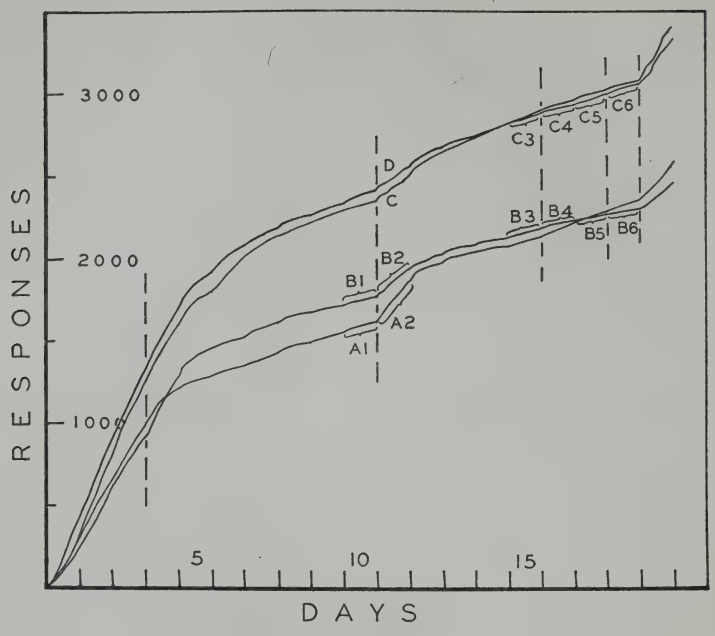

FIGURE 3

becomes fully developed, but the characteristics of the records for reversal are apparent in spite of the considerable reduction required by the figure. The following five days show the development of the second discrimination, at the end of which the rate of elicitation of the extinguished member ($S_{AB..L..}$—R) has reached a low value. (As we shall note again later, it is not as low as at the end of the first discrimination.)

In order to understand this second curve we must consider an experiment involving an original discrimination in which no previous periodic reconditioning takes place. Four rats were conditioned to the lever in the presence of the light and the responses were then extinguished both in the light and in the dark. The rats were then transferred directly to the discriminative procedure without the usual three or four daily hours of periodic reconditioning. A typical set of records is reproduced in Figure 4. The first record shows some upward convexity at the beginning, which is due to "loss of extinc-

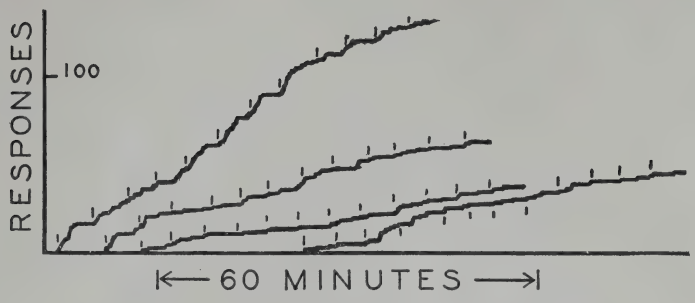

FIGURE 4

tion" (4). There is then a quite significant acceleration, and a subsequent decline in rate. The next three days show the usual development of a discrimination. The fourth record shows a slight burst of activity shortly after the start and the total for the day is consequently high; but these are incidental factors that have already been described as characteristic of the process (1). The complete sets for all four rats and their average (the heavy line) are given in Figure 5; the series from Figure 4 is marked *D*.

The fairly high rate developed on the first day in this simple experiment is for ($S_{AB..}$—R), *which has never been reinforced.* The strength which it develops must be due to induction from the reinforcement of ($S_{AB..L..}$—R). Part of the effect is the result of the

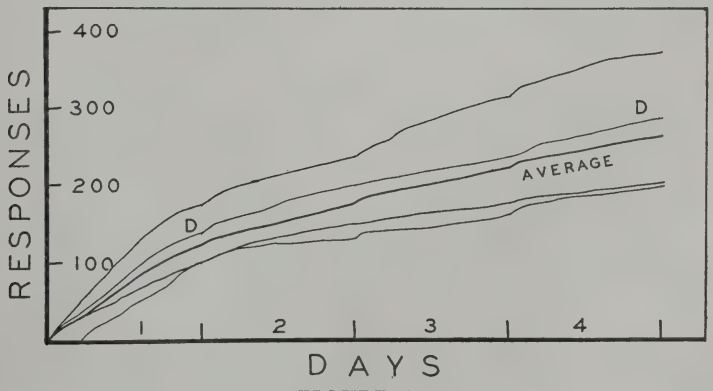

FIGURE 5

original conditioning, which is reflected in the "loss of extinction" at the beginning of the first records. That is to say, the extinction of both ($S_{AB..}$—R) and ($S_{AB..L..}$—R) has not wholly eliminated the inductive effect upon ($S_{AB..}$—R) of the original conditioning of ($S_{AB..L..}$—R). The *positive* acceleration in the first record in Figure 4 is due, on the other hand, not to a loss of extinction, but to the concurrent periodic reinforcement of ($S_{AB..L..}$—R).

This is not exactly the sort of curve that we should expect to find after reversal of a discrimination, but it is very close to it. What appears in Figure 4 as "loss of extinction" is original extinction in Figure 2. But both curves exhibit positive acceleration and for the same reason—induction due to the concurrent periodic reinforcement of a related reflex. The negative acceleration is in both cases the beginning of a discrimination. The only characteristic here important is that the curves in Figure 5 inclose a much smaller area than the first curves in Figure 3 or than any curve for discrimination after periodic reconditioning, other conditions being equal. For a similar reason we should not expect the area of the curve for discrimination *after reversal* to resemble that of the original curve. The original curve includes the extinction of the effect of three hours of periodic reconditioning; the reversed curve includes, in addition to the extinction of ($S_{AB..L..}$—R), only as much of an inductive effect as can take place before the new discrimination becomes effective.[2] Since the areas are not comparable, the most significant effect of the reversal is the shape of the records for the first day, which reveals clearly enough the nature of the change taking place.

III

But there is another important effect of reversal—upon the state of the reinforced reflex. The records of the rate of responding that we have considered supply information only about the unreinforced member. The reinforced responses occur at regular intervals and contribute to the records only a low constant slope, which could be subtracted if we wished, but which we are disregarding. Significant information about the reinforced member may be obtained,

[2]A study of a discrimination beginning *ab initio*, without previous conditioning of either stimulus, will be published elsewhere (6). The result is in agreement with the present interpretation of a discrimination without periodic reconditioning.

however, from its latency, which is measured (in the present case to the nearest second) as the interval elapsing between the introduction of the light and the appearance of the response.

Before the original discrimination develops, the value of this latency is determined by the basic rate under periodic reconditioning (1, Appendix III). At the beginning of the discrimination the latency drops rapidly to a value of the order of six or seven seconds (the individual differences being rather large). A nearly minimal value may be reached within four or five reinforcements, but since the interval depends at the start on accidental factors, it is difficult to trace the course of this change. The average latency for the first day of the discrimination (comprising either nine or eleven reinforcements depending upon whether the interval is six or five minutes) is already well below the average value obtaining under periodic reconditioning. There is a slower daily fall thereafter, which may persist in some slight degree as long as the experiment is carried on (for at least 30 days).

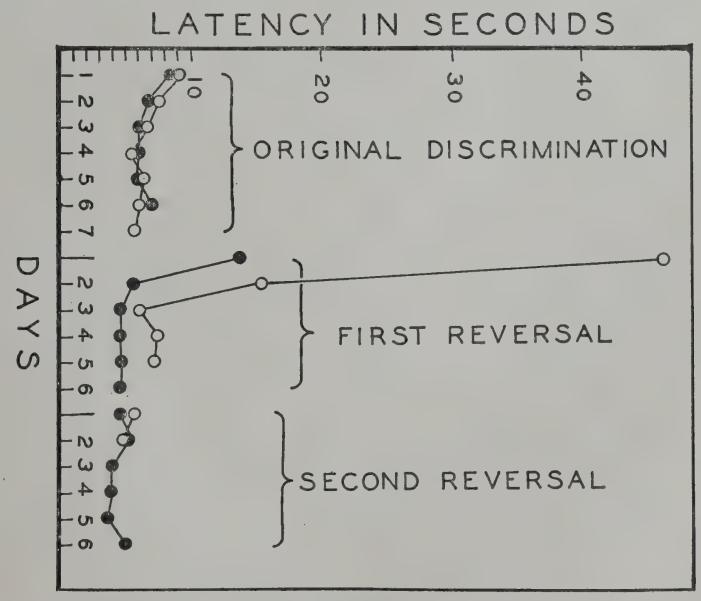

FIGURE 6

The average latencies for the eight rats in the present experiment (nine latencies per rat per day) are represented by the open circles in Figure 6. The series for the original discrimination [latencies for ($S_{AB..L..}$—R)] begins at the already reduced value of 9.2 seconds. A significant drop is to be observed during the following two or three days. After the reversal the latency is measured for ($S_{AB..}$—R), which has previously been extinguished and is therefore at a very low strength. The latency begins, consequently, at an extremely high value. The average for the first day is 46.0 seconds. The reconditioning is slow, and the effect of the previous extinction is still apparent on the second day where the latency has fallen only to 15.5 seconds. On the third day, however, the strength reaches approximately the value held by ($S_{AB..L..}$—R) in the original discrimination and continues approximately at that value thereafter.

IV

If we now reverse the discrimination a second time, we return, of course, to the original set of conditions. There seems to be no reason to expect *a priori* that this will not involve another complex curve of the type already obtained. ($S_{AB..}$—R) has now been reconditioned and should be extinguished, although the curve for extinction might be considerably smaller (4, 5). ($S_{AB..L..}$—R) is at a low strength and should return to essentially its strength under periodic reconditioning. The experimental result, however, which is so far without exception, shows no effect of a second reversal. In the present experiment the reversal was made after the new discrimination had been in force for five days. Two pairs of records for the days immediately before and after the second change are given in Figure 7, and their position in Figure 3 is indicated by brackets. It is apparent in comparing these records with Figure 2 that there is no initial increase in rate on the day of reversal that could be regarded as an extinction curve for ($S_{AB..}$—R) nor is there any positive acceleration with subsequent decline in rate. Some slight effect appears if we average the total number of responses per hour for all eight records. The averages for the three days prior to the second reversal and the two days following it are as follows:

.....96, 92, 88 | *95,* 79.....

Toward the end of the discrimination the slope was falling at the

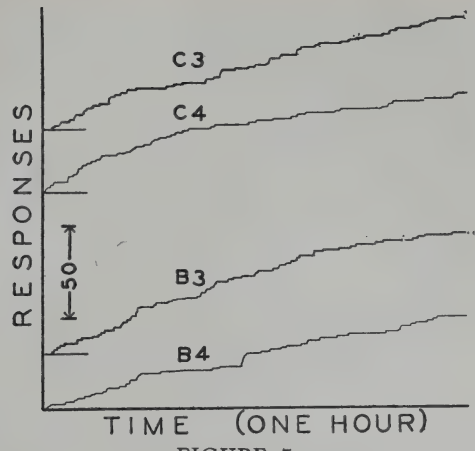

FIGURE 7

rate of about four responses per hour. On the day of reversal there was an increase of seven responses (eleven above the expected number), but on the second day the rate had dropped to approximately what it would have been without reversal. The only significant change, then, (if this is really to be taken as significant), is a slight increase in rate, apparently distributed evenly throughout the hour.

Similarly there is no change in latency comparable with that observed upon a first reversal. This is apparent in Figure 6 where the latencies are given for two days after the second reversal. (There is an apparent omission of a day at the end of the second series because the graph has been spaced out to accommodate a second set of data to be described shortly. The three series were taken continuously.) It will be seen that the rise in latency at the first reversal has no counterpart at the second.

Upon changing the conditions a third time (returning now to the reversed set) we again obtain no effect, as shown at the fourth vertical line in Figure 3. Two pairs of records are given in Figure 8 and their positions in the series are indicated in Figure 3 as before. The number of responses on the day of the third reversal was 76, which follows perfectly in the declining series given above. The latency also undergoes no significant increase, although this is not shown in the figure.

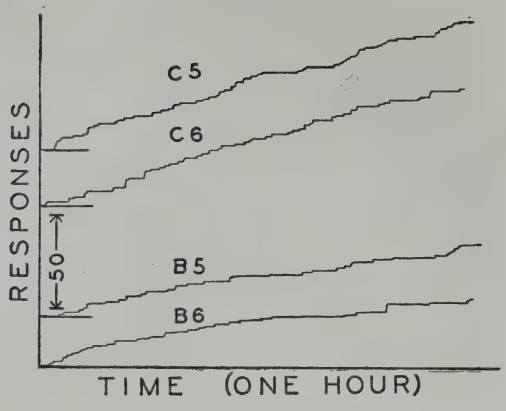

FIGURE 8

At the end of the experiment a check was made against a possible decline in rate for some unknown reason by returning to simple periodic reconditioning. The recovery of a constant slope is shown on the last day of Figure 3. It is clear that a significantly high strength is quickly developed. The average slope is not, however, equal to that originally observed, and it is in general true that after prolonged discrimination the acceleration toward the normal slope under periodic reconditioning is retarded. We have already seen examples of this in records *C* and *D* in Figure 3, although the effect might there have been due to the reversal. Four other cases, obtained after a long series of discriminations to be described shortly, are given in Figure 9. A typical curve (*K*) for return to the

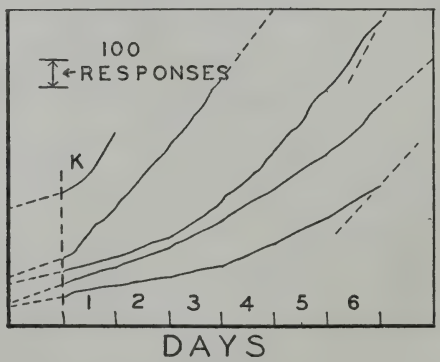

FIGURE 9

periodic slope after a short discrimination is included on the same coordinates [the original curve is reproduced in (5) Figure 2]. The broken lines at the beginning give the slopes reached at the end of the discrimination. Those at the end give the original slopes under periodic reconditioning. It will be seen that only one of the four rats reaches its original slope on the first day. The others show fairly smooth but greatly retarded accelerations, and one at least has not reached the required slope by the end of the sixth day.

V

In acounting for the difference between the effects of a first and a second or third reversal, a partial explanation may be drawn from information already at our disposal. So far as the first limb of the curve (Figure 2, records *A2* and *B2*) is concerned, it has been shown that extinction after discrimination may yield a smaller curve than original extinction (5), and it is not unreasonable to suppose that a second curve would average no more than seven responses in one hour and that a third would be negligible. The absence of a positively accelerating limb may be referred to the retardation in Figure 9 and to the assumption that, if the acceleration can be put off until a new discrimination has developed, it will not occur. But these explanations are obviously labored, if not definitely inadequate. It is clear that a second reversal differs from the first in a special way, and if we define the process of discrimination rather narrowly as the breakdown of induction, we can distinguish between them by saying that at the second reversal a new discrimination is not established.

This might reasonably have been expected. We have already destroyed the inductive effect of $(S_{AB..L..}—R)$ upon $(S_{AB..}—R)$, at least to the degree given by the slope of the discrimination curve when it was brought to an end. That this has not affected the reciprocal effect of $(S_{AB..}—R)$ upon $(S_{AB..L..}—R)$ is obvious from the result of the first reversal; the reciprocal induction must be broken down separately. But we have no reason to suppose that during this second process, the result of the first will be undone. [Indeed, it is not apparent how induction can possibly be affected in a positive direction, except through a (relatively slow) spontaneous recovery.] Accordingly, when we reverse the conditions a

second time, we return to a discrimination that has already been established and no further change is needed.

But the assumption that the breakdown of induction is irreversible will not wholly account for our present observations. Granted its validity, we should still expect a second reversal to produce (*a*) the extinction of the previously reinforced member (observed as a small extinction curve on the day of reversal) and (*b*) the reconditioning of the previously unreinforced member (observed as a shortening of its initially long latency). The requirement of a small extinction curve is approximately satisfied by our eleven extra responses at the second reversal, but not at later reversals, and the requirement of an initially long latency with subsequent reduction is not satisfied in any case after the first reversal. Thus in Figure 6 we have already seen that no significant increase in latency accompanies a second reversal. This is true not only of the average for the hour but of the first few reinforcements. In another series of experiments (to be described shortly) a reversal was made in the middle of the hour in order to show the simplicity of this change. Four typical records showing three successive days for each rat are given in Figure 10. The upper record in each group shows the last day of a discrimination in which $S_{AB..}$ is being reinforced, the middle record shows the first day of (a third) reversal, and the lower record a continuation of the reversed discrimination until the middle of the hour, when the conditions were reversed again. On this last day the light was turned on as usual before the reinforced response at the vertical line. It was then *left on* for the succeeding interval. When it was turned off again a response followed immediately, and it was then turned on. During the rest of the hour $S_{AB..}$ was periodically reinforced. The records show no significant change correlated with this reversal. The average latencies for eight rats for the eleven reinforcements during the hour (omitting the reinforcement at release) are as follows:

6.0, 3.4, 5.3, 5.6, 3.0, | 6.4, 4.4, 5.7, 7.0, 5.4, 5.0

The latency of 6.4, for the first discriminatory response after reversal, cannot be taken as significantly above the average.

A more crucial experiment on this point was made with the same

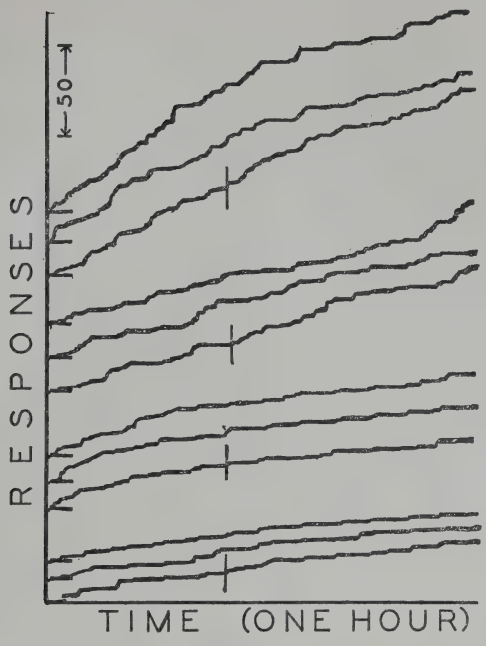

FIGURE 10

eight rats on a later day, when the conditions of the discrimination were changed at every interval during the hour. At release the light was on and the first response reinforced; the light was left on for five minutes, then turned off. A response immediately followed. The light was left off for five minutes, then turned on, when another response followed—and so on. In one group of four rats the average latency for the hour was 5.1 seconds as compared with five seconds for the previous day under normal discriminatory conditions. In the other group the latency was seven seconds as compared with five seconds on the previous day. The increase in the latter case was due to five especially long latencies (averaging about 25 seconds each) which appeared anomalously in the records of two of the rats. While they are probably significant, they do not seriously affect the present conclusion, which is that an increase in latency such as would be required by a theory of rapid reconditioning at each new discrimination is not observed. Similarly the slopes

of the eight records show no effect of extinction, such as would also be required; the averages for the two days prior to the day of repeated reversal were 135 and 116 responses respectively. On the day of reversal the average was 110 responses. (These relatively high values are due to the late stage of the experiment. As we have already noted, the slopes approached by later discrimination curves progressively increase.)

VI

In accounting for this experiment we reach at last the point of the present paper. It is obvious that at a second reversal and thereafter the effective basis for the discriminatory response is the *change* from either stimulus to the other. The observed fact is that both $S_{AB..\lambda \rightarrow L..}$ and $S_{AB..L \rightarrow \lambda..}$ are effective stimuli, while $S_{AB..L..}$ and $S_{AB..}$ are ineffective. This is the condition that we originally wished to set up in making a change the effective basis for a discrimination, and our only remaining problems are to explain, first, why the condition is found already established at the second reversal and, second, why there is no inductive influence of $(S_{AB..\lambda \rightarrow L..}—R)$ upon $(S_{AB..L..}—R)$ nor of $(S_{AB..L \rightarrow \lambda..}—R)$ upon $(S_{AB..}—R)$—or, in other words, why the discriminations between $S_{AB..\lambda \rightarrow L..}$ and $S_{AB..L}$ and between $S_{AB..L \rightarrow \lambda..}$ and $S_{AB..}$ are found already in existence after discriminations between $S_{AB..L..}$ and $S_{AB..}$ have been set up in both directions.

The first point is not difficult. In the original discrimination the reinforced response is really to $S_{AB..\lambda \rightarrow L..}$ (if we are now to make this distinction throughout). The extinction, on the other hand, is of the responses to S_{AB}, not $S_{AB..L \rightarrow \lambda..}$, since the rat is eating for at least 15-20 seconds after the change back to $S_{AB..}$ is made. After the first reversal $(S_{AB..L \rightarrow \lambda..}—R)$ is periodically reinforced and $(S_{AB..L}—R)$, *not* $(S_{AB..\lambda \rightarrow L..}—R)$, is extinguished. The second of the responses to S_{AB}, not $S_{AB..L \quad \lambda..}$, since the rat is eating for reversal therefore finds $(S_{AB..\lambda \rightarrow L..}—R)$ and $(S_{AB..L \rightarrow \lambda..}—R)$ previously reinforced and not subsequently extinguished, and $(S_{AB..}—R)$ and $(S_{AB..L..}—R)$ extinguished and not subsequently reinforced, which is the required condition.

The second point, the lack of induction, is apparently not to be accounted for on any previously established principle. We may leave

it, therefore, in the form of a descriptive statement as one of the results of the present study: with the present technique, in which reinforcement occurs very soon after presentation of the stimulus, it is possible to obtain a discrimination based wholly upon a change (as we here define it) by exhausting possible discriminations between the stimuli themselves.

[That a stimulus can depend upon the temporal proximity of another stimulus for its effectiveness has already been shown (1). No significant difference as a discriminative component was found between a light which remained on during reinforcement and a sound which was presented *before* reinforcement but never simultaneously with it. If we write the sound as $S_{AB..\lambda \rightarrow L \rightarrow \lambda..}$, its relation to the present "change" will appear. A different formulation is required in the case of the change because of other possible bases for the discrimination.]

VII

In another series of experiments an attempt was made to separate the factors in the curves for the day of reversal by interpolating a day of periodic reconditioning. The records for a group of eight male rats (90 days old at the start) have been averaged to give the lower curve in Figure 11, in which the average curve for the

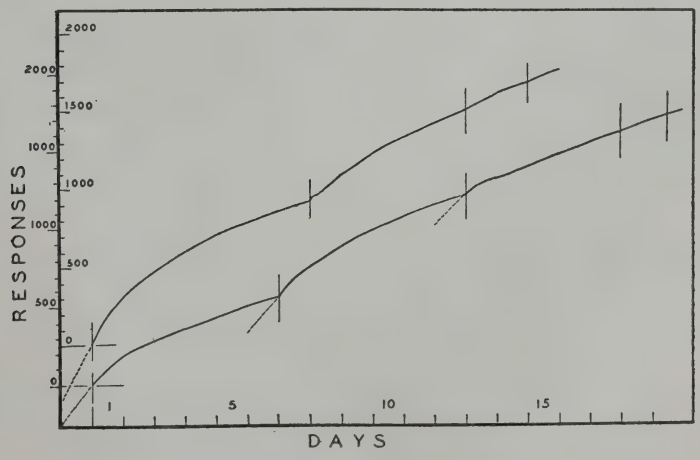

FIGURE 11

eight rats in the preceding experiment is also given. To aid the comparison the interpolated days of periodic reconditioning have been omitted from the main curve and inserted beneath the last days of the preceding discriminations.[3] To go over the curve in detail—the first day shows the average slope for the last of four days of periodic reconditioning (of $S_{AB..}$—R) at intervals of five minutes. [The slope is somewhat less than for the other group. Individual variation—in the ratio N_e/N_c (1)—is great enough to account for differences in the averages of small samples, but we may also appeal to slight differences in methods of feeding, in health, atmospheric conditions, and so on, which may vary to some extent from one experiment to another although they remain practically constant during a single experiment.] The following six days of the figure show the development of a discrimination, ($S_{AB..}$—R) being extinguished. On the seventh day $S_{AB..L..}$ is present continuously, and responses to it are periodically reinforced; the recovered slope is indicated with a broken line on the sixth day of the figure. The separate records resemble Figure 1[4] the positive acceleration approaching a straight line. The total slope is somewhat less than that observed before discrimination, but this is mainly due to the time required for acceleration rather than to the retarded acceleration noted above. The reversed discrimination begins on the following day from the new periodic slope, and is well developed at the end of six days. Another day of periodic reconditioning (of $S_{AB..}$—R) follows, and the resulting slope is shown with the broken line on the twelfth day. The retarded acceleration is now beginning to be felt.

This second interpolated day of periodic reconditioning forces the reconditioning of ($S_{AB..}$—R), which could not occur in the first experiment because of the lack of induction. Accordingly, when the discrimination is reversed again, a significant increase in slope is observed. The average number of responses for the three days prior

[3]Broken lines give average slopes only. No attempt has been made to show the initial positive acceleration.

[4]All records of periodic reconditioning, particularly on the first day, are subject to chance variations depending upon whether or not the rat responds soon after the magazine has been turned on. If it does not (and this is not under our control) an orderly periodic reinforcement is, of course, impossible. Only rarely is the general course of the record obscured.

to the day of periodic reconditioning, the average for the latter (in parentheses), and the average for three days of the new discrimination are as follows:

85, 72, 62 | 192 | *99, 85, 80*

The extra responses after reversal are chiefly due to the periodic reconditioning of ($S_{AB..}$—R) on the interpolated day. That they are not supplemented by any considerable induction from ($S_{AB..L..}$—R)—that no new discrimination is taking place—is clear from the quick adjustment to a constant slope, though this slope, as we have already noted, is increased by the reversal.

The average latencies for this series of experiments are given as the solid circles in Figure 6. There is a significant increase at the first reversal, as before, but none at the second. The increase at the first is much less than in the previous experiment, but this can reasonably be regarded as the effect of the day of periodic reconditioning.

As a control, the conditions were reversed a third time without returning to the periodic slope. The result is shown in Figure 11 at the fourth vertical line and fully confirms the previous finding. On the following day the conditions were reversed in the middle of the hour as already described (Figure 10).

Figure 11 shows very well the progressively higher slope approached by successive discrimination curves. If we combine this phenomenon with the retardation of acceleration shown in Figure 9, we may describe both changes together as a tendency toward stabilization at a mean rate. The significance of this change is not at present apparent. It does not seriously interfere with the present conclusions.

VIII

An experiment of this sort must be controlled against the possibility that a discrimination in which the reinforced member is $S_{AB..L..}$ may differ significantly from one in which it is $S_{AB..}$. Otherwise the effect of the first reversal may in part be due to a change in the kind of process. In the present case the control was supplied by dividing the animals between the two types. Half the cases reported above were actually of the opposite sort, and in these

cases L should be read λ and *vice versa*. In Figures 2, 4, 5, and 10 all records were as described. In Figure 3 the two lower curves were as described, the two upper were the opposite; the apparent correlation, which is deceptive, is due to the periodic slope which happened to be greater in the latter case. In Figures 6 and 11 each point or curve represents an equal number of records of each type. The cases of Figures 7 and 8 can be found from Figure 3.

No significant difference is to be observed between the two cases which is great enough to disturb the present conclusions. The eight cases in the lower curve in Figure 11 are convenient for comparison because each case includes three discrimination curves of equal length (six days). Sorting these 24 curves into the two kinds and averaging, we obtain Figure 12. The broken lines are for the slopes under periodic reconditioning. In the curve *A* the light was off during periodic reconditioning and during most of the discrimination—that is, (S_{AB}.—R) was the extinguished member. In *B* the opposite was true. The difference between the curves is mainly attributable to the difference in the initial periodic slopes. There is no significant effect upon latency. Where the change prior to reinforcement was $L \rightarrow \lambda$, the latency was 5.86; where it was the opposite, the latency was 5.39.

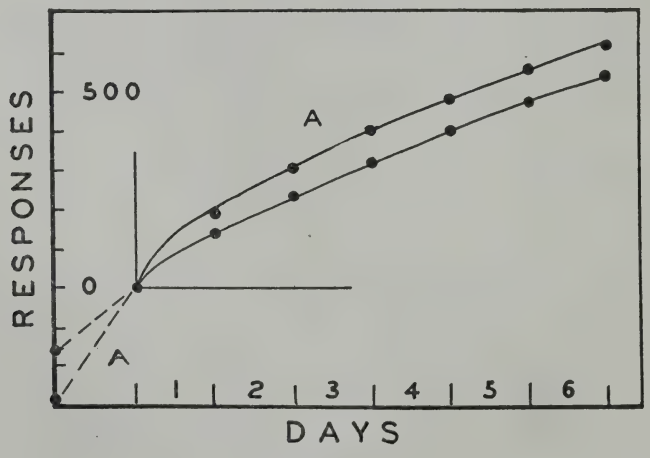

FIGURE 12

Summary

1. A change in the properties of a stimulus may be formulated by writing the stimulus to which the change is made (say, $S_{AB..L..}$) and adding an expression for the temporal proximity of the stimulus from which it is made (giving $S_{AB..\lambda \rightarrow L..}$, where λ = the absence of or some other value of the property L). A discrimination based upon the change alone must be between $S_{AB..\lambda \rightarrow L..}$ and $S_{AB..L..}$ or between $S_{AB..L..\rightarrow \lambda..}$ and $S_{AB..}$ and requires for its demonstration the elimination of possible discriminations between $S_{AB..L..}$ and $S_{AB..}$.

2. A discrimination between $S_{AB..L..}$ and $S_{AB..}$ is first established, the response to the latter being extinguished. The conditions are then exactly reversed. The result is that ($S_{AB..L..}$—R) is extinguished, ($S_{AB..}$—R) is reconditioned, and a discrimination in the opposite direction is set up. The experimental curve for the day of reversal gives the resultant of these three processes.

3. The effect of reversal upon the latency of the reinforced reflex is also reported.

4. When the conditions are reversed a second time there is no comparable change in either rate of responding or latency.

5. This can be partly accounted for by assuming that the breakdown of induction which characterizes the process of discrimination is irreversible. But on this assumption each reversal will require reconditioning of the previously extinguished member and extinction of the previously conditioned. No significant evidence of these effects is found in crucial experiments.

6. The following interpretation is therefore demanded. At a second reversal, and thereafter, the effective stimuli are $S_{AB..\lambda \rightarrow L..}$ and $S_{AB..L..\lambda..}$, while $S_{AB..L}$ and $S_{AB..}$ are not effective. That is to say, we have a discrimination between the stimuli which involve the temporal proximity of other stimuli and the same stimuli lacking that proximity. But this is our original formulation of the way in which a "change" can become the effective basis for a discrimination.

7. A confirming experiment is reported in which interpolated days of periodic reconditioning are used to separate out the several factors operative on the days of reversal.

8. A control against a possible difference in the discriminations between $S_{AB..L..}$ and $S_{AB..}$ and *vice versa* is described.

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UNE DISCRIMINATION BASÉE SUR UN CHANGEMENT DANS LES PROPRIÉTÉS D'UN STIMULUS

(Résumé)

1. On peut formuler un changement dans les propriétés d'un stimulus au moyen d'écrire le stimulus *auquel* le changement est fait (par. ex. $S_{AB..L}$.) et d'ajouter une expression pour la proximité temporelle du stimulus *duquel* il est fait (ce qui donne $S_{AB..\lambda \rightarrow L..}$, ou λ = l'absence ou une autre valeur de la propriété L). Il faut qu'une discrimination basée sur le changement seul soit entre $S_{AB..\lambda \rightarrow L..}$ et $S_{AB..L..}$ ou entre $S_{AB..L \rightarrow \lambda..}$ et $S_{AB..}$ et elle exige pour sa démonstration l'élimination des discriminations possibles entre $S_{AB..L..}$ et $S_{AB..}$.

2. Une discrimination entre $S_{AB..L..}$ et $S_{AB..}$ est premièrement établie, la réponse à la dernière étant éteinte. Les conditions sont ensuite exactement renversées. Le résultat est que ($S_{AB..L..}$ —R) est éteinte, ($S_{AB..}$ —R) devient de nouveau conditionelle, et une discrimination est établie dans la direction contraire. La courbe expérimentale pour le jour du reversement donne le résultat de ces trois processus.

3. On rapporte aussi l'effet du renversement sur la latence du réflexe renforcé.

4. Quand les conditions sont renversées une seconde fois, il n'y a aucun changement comparable dans la vitesse de la réponse ou dans la latence.

5. On peut expliquer ceci en partie, en supposant que l'écroulement de l'induction lequel caractérise le processus de la discrimination ne soit pas renversable. Mais avec cette supposition chaque renversement exigera que le membre antérieurement éteint soit de nouveau conditionnel et que le membre antérieurement conditionnel soit éteint. On ne trouve aucunes évidences de ces effets dans des expériences définitives.

6. On exige donc l'interprétation suivante. A un deuxième renversement et après, les stimuli effectifs sont $S_{AB..\lambda \rightarrow L}$ et $S_{AB..L \rightarrow \lambda..}$. Mais $S_{AB..L..}$ et S_{AB} ne sont pas effectifs. C'est-à-dire, on a une discrimination entre les stimuli ou il s'agit de la proximité temporelle d'autres stimuli et les mêmes stimuli qui n'ont pas cette proximité. Mais ceci est notre formulation originale de la méthode selon laquelle un "changement" peut devenir la base effective pour une discrimination.

7. On rapporte une expérience de confirmation où les jours interpolés du conditionnement nouveau périodique sont employés pour séparer les plusieurs facteurs qui opèrent les jours du renversement.

8. On décrit un contrôle contre une différence possible dans les discriminations entre $S_{AB..L..}$ et $S_{AB..}$ et *vice versa*.

SKINNER

EINE UNTERSCHEIDUNG, DIE AUF EINER VERÄNDERUNG DER EIGENSCHAFTEN EINES REIZES BERUHT

(Referat)

1. Eine Veränderung der Eigenschaften eines Reizes kann durch das Schreiben des Reizes, auf den die Veränderung folgt (z. B. $S_{AB..L..}$) formuliert werden, und durch den Zusatz eines Ausdrucks für die zeitliche Nähe des Reizes, von der er gegeben wird ($S_{AB..\lambda \rightarrow L..}$, wo λ die Abwesenheit der Eigenschaft oder irgendeinen anderen Wert dieser Eigenschaft bedeutet). Eine Unterscheidung, die auf der Veränderung allein beruht, muss zwischen $S_{AB..\lambda \rightarrow L..}$ und $S_{AB..L..}$ oder zwischen $S_{AB..L \rightarrow \lambda}$ und $S_{AB..}$ sein und erfordert zu ihrem Beweis die Ausschaltung der möglichen Unterscheidungen zwischen $S_{AB..L..}$ und $S_{AB..}$.

2. Eine Unterscheidung zwischen $S_{AB..L..}$ und $S_{AB..}$ wird zuerst festgesetzt, die Antwort auf die letztere wird ausgeschaltet. Die Zustände werden dann genau umgestellt. Die Folge davon ist, dass ($S_{AB..L..}$ —R) ausgeschaltet, ($S_{AB..}$ —R) wiederbedingt, und eine Unterscheidung in der umgekehrten Richtung aufgestellt wird. Die experimentelle Kurve für den Tag der Umstellung gibt die Resultante dieser drei Vorgänge.

3. Die Wirkung der Umstellung auf die Verborgenheit des verstärkten Reflexes wird auch angegeben.

4. Wenn die Umstände zum zweiten Mal umgestellt werden, gibt es keine vergleichbare Veränderung, weder in der Schnelligkeit der Antwort noch in der Verborgenheit.

5. Dies kann teilweise durch die Annahme erklärt werden, dass der Zusammenbruch der Induktion, der den Vorgang der Unterscheidung charakterisiert, nicht umkehrbar sei. Aber bei dieser Annahme wird jede Umstellung das Wiederbedingen des vorher ausgeschalteten Mitglieds und das Auslöschen des vorher Bedingten erfordern. Kein bedeutsamer Beweis für diese Wirkungen kann durch kritische Experimente nachgewiesen werden.

6. Die folgende Deutung wird daher verlangt. Bei einer zweiten Umstellung und nachdem sind die wirksamen Reize $S_{AB..\lambda \rightarrow L}$ und $S_{AB..L \rightarrow \lambda..}$ Aber $S_{AB..L..}$ und $S_{AB..}$ sind nicht wirksam. Das heisst,

wir haben eine Unterscheidung zwischen den Reizen, die von der zeitlichen Nähe der anderen Reize und derselben Reize, die jener Nähe ermangeln, abhängen. Aber dies ist unsere ursprüngliche Formulierung der weise, in der eine "Veränderung" der wirksame Grund für eine Unterscheidung werden kann.

7. Ein bestätigendes Experiment wird angegeben, in dem eingeschobene Tage des periodischen Bedingens gebraucht werden, un die verschiedenen Faktoren abzusondern, die an den Tagen der Umstellung wirksam sind.

8. Eine Kontrolle für einen möglichen Unterscheid in den Unterscheidungen zwischen $S_{AB..L..}$ und $S_{AB..}$ und umgekehrt wird beschrieben.

SKINNER

INDIRECT CONDITIONING*[1]

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INTRODUCTION

The present investigation developed gradually from an attempt to settle this question: After two previously "neutral" stimuli have been independently conditioned to the same response, will the subsequent reconditioning of one to a second response render the coconditioned one also capable of evoking this second response? Two lines of evidence point toward such a possibility. One is the factor of training transfer that apparently takes place between two stimuli which have been independently conditioned to the same response. This has been demonstrated in connection which *inhibition* by Pavlov (6) and by the writer (10) in the phenomenon of *secondary extinction,* which is a reduction of the effectiveness of a conditioned stimulus resulting from the repeated non-reinforced presentation of its coconditioned stimulus. It has also been demonstrated in connection with facilitation or reinforcement by the writer (10) in the phenomenon of *secondary reinforcement,* which is the reinstatement of the original effectiveness of an extinguished conditioned stimulus through the direct reinforcement of its coconditioned stimulus. The other line of evidence is the factor of training transfer that apparently takes place between a conditioned stimulus and its unconditioned (conditioning) stimulus, which was demonstrated by the writer (9).[2] It was found in humans that, after a very faint flash of an electric light had been paired a number of times with a sudden rap on the cheek, which served to evoke a wink, and the rap on the cheek had then been paired repeatedly with a shock evoking

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[1]The writer wishes to express his appreciation to Professor Clark L. Hull under whose direction this investigation was carried out.

[2]A similar phenomenon has been reported by Prokofiev and Zeliony (7).

withdrawal of the finger, the flash served to evoke a withdrawal of the finger in the majority of subjects.[3]

These two lines of evidence, viz., the training transfer between homogeneous conditioned reactions, and the acquisition of a new response by a conditioned stimulus through subsequent conditioning of its unconditioned stimulus, suggested the possibility of indirectly attaching a new response to one of two stimuli conditioned to a given response by reconditioning the other one to a second response. The object of the present investigation was to examine this possibility.

The two responses chosen were (1) withdrawal of the finger and (2) the eyelid reaction or wink. A faradic shock on the pad of the right index finger served as the unconditioned stimulus for the former. A blow on the cheek just below the right eye, which will hereafter be referred to as the strike, was used as the unconditioned stimulus for the latter. The faint flash of an electric light served as one extra stimulus; the faint sound of an electric buzzer served as the other. The specific problem was to determine whether, by presenting the flash and buzz alternately with the strike a number of times and then following this by an establishment of conditioning between the flash and withdrawal of the finger by pairing the flash with the shock, the buzz could be made to produce withdrawal of the finger.

Apparatus, Subjects, and Instructions

The apparatus, with the following addition, was identical with that already described in connection with a previous experiment (9), to which the reader is referred. The sound of an electric buzzer, audible above the hum of the masking head phones but dampened so as to produce at most only a minimal wink, was added to the battery of automatically activated stimuli. The subjects were all male college students, and the instructions were the same as in the previous experiment.

Procedure and Results

The first group of subjects, group A, consisting of 10 subjects, was put through a procedure extending over three successive days.

[3]The findings of this experiment will be considered in the interpretation of the present results, as the two investigations appear to be intimately related.

During the first two days the flash and buzz were independently conditioned to the winking reaction by being paired with the strike. On the third day the flash was reconditioned, this time to the finger-withdrawal reaction, by being paired with the shock. Subsequent to this the buzz was tested to determine whether the conditioning had carried over to it, that is, to determine whether the buzz would produce withdrawal of the finger. The program of stimulation for this group was as follows:

First day

1 presentation of the flash alone.
1 presentation of the buzz alone.
14 presentations of the flash and strike } alternated according to
14 presentations of the buzz and strike } prearranged random order.

Second day

14 presentations of the flash and strike } alternated according to
14 presentations of the buzz and strike } prearranged random order.

Third day

3 presentations of the flash and strike } alternated according to
3 presentations of the buzz and strike } prearranged random order.
25 presentations of the flash and shock
2 presentations of the buzz.

Wherever a double stimulation was given the conditioned stimulus always preceded, and the interval between the two components was approximately .3 second.[4] The interval between successive stimulations was always 38.5 seconds. The order of stimulation for the respective days, in which *f* represents the unreinforced flash, *F* the reinforced flash, *b* the unreinforced buzz, and *B* the reinforced buzz, were as follows:

[4]Post experimental photographic determination showed that where the flash-strike combination was used the interval between the onset of the flash (under increased illumination) and that of the strike was .32 second, the duration of the flash being .26 second and the interval between the termination of the flash and onset of the strike being .06 second. Where the buzz-strike combination was used the interval between the onset of the buzz and that of the strike was .32 second, the duration of the buzz being .28 second and the interval between the termination of the buzz and onset of the strike being .04 second. Where the strike-shock combination was used the interval between the onset of the strike and that of the shock was .32 second, the duration of the shock being approximately .38 second. These temporal relations were undoubtedly subject to some degree of variability due to wearing of the contact points, slight errors in their adjustment, and variability in the strike-releasing mechanism.

First day

f b F B F B B B F F B F B F F F B B F B B F F B B B F F F B

Second day

F B F B B B F F B F B F F F B B F B B F F B B b F F F B

Third day

F B B F F B F F F F F F F F F F F F F F F F F F F F F b b

Six of the ten subjects in this group gave finger-withdrawals to the flash at the end of the third session.[5] Control Group B was run to determine whether this might not be the result of irradiation instead of a carrying over of the conditioning to the former homogeneous stimulus. This group was treated in exactly the same manner as Group A, with the one exception that wherever the buzz-strike combination was given Group A, the strike alone was given Group B. Thus there was no reinforcement of the buzz. Should responses to the buzz alone in this group occur at the end of the third session, it would be evident that the result could not be considered a pure conditioning phenomenon.

None of the subjects of Group B gave responses at the crucial point. It was therefore assumed, at the time, that the responses given by the subjects in Group A were due to carrying over of the conditioning from the flash to the buzz which resulted from previously associating them with a common response. To make certain of this interpretation, it was decided that another control group should be run, in which there would be established first a conditioning between the buzz and eyelid reaction and then a conditioning between the flash and finger-withdrawal. If under these conditions the buzz, after the establishment of the second connection, should evoke a finger-withdrawal it would be necessary, because of the lack of any apparent common bond between the flash and buzz, to invoke some other interpretation of the results. Thus Group C was put through exactly the same procedure as Group A, with the one exception that wherever in Group A the flash-strike combination was given, in Group C the strike was given alone. This pro-

[5] As the extent of the finger tracings could not be considered a very direct measure of either the direction or extent of finger movement, the records were treated in a qualitative way only. Any deflection on the record was considered a response, and no distinctions were made between amplitudes of these deflections.

cedure somewhat unexpectedly resulted in seven out of ten subjects in this group giving positive responses at the crucial point.

The interpretation of such a result was by no means clear. It seemed possible, though not probable, that the phenomenon was due to some general irradiation which had not occurred in Group B because the buzz, having been presented but once at the very beginning of the experiment and not again until tested two days later, might have been too foreign to the general situation to produce this irradiation. Had the buzz been given frequently during the experiment and, though not reinforced, been allowed to become associated with the entire situation, it might perhaps have evoked a finger-withdrawal at the test point. To investigate this possibility Control Group D was run.

This group, consisting of ten subjects, was run through two experimental sessions, one on each of two successive days. The stimuli and intervals used in this portion of the experiment were the same as those used in connection with the other groups. The first day's program consisted of thirty successive presentations of the buzz alone. No record of responses was taken during this session. The second day's program consisted of twenty-five successive presentations of the flash-shock combination followed by two tests of the buzz alone. A record was taken during the second session. None of the subjects of this group showed responses at the crucial point.

Another possibility suggested itself. It seemed possible that after repeated faradic stimulation of the finger, a set might be established so that the strike would produce a finger response where it had produced none before. If such were the case it would also seem likely that a stimulus which had been associated with the strike might under this condition likewise elicit a finger response. To test this possibility, Group E, consisting of ten subjects, was run.

Group E was also put through two experimental sessions, one on each of two successive days. The stimuli and intervals used here were the same as those used in connection with the other groups. The first day's program consisted of twenty-five successive presentations of the strike alone. No record was taken during this session. The second day's program consisted of five successive presentations of the strike alone, twenty-five presentations of the shock

alone, and finally two more presentations of the strike alone. One member of this group gave a finger response at the first presentation of the strike at the end of the experiment. All the rest gave negative results.

As no group had yet been run in which the strike was accompanied by an extra stimulus and the shock was not, the need of such a group was felt. Therefore Group F, consisting of ten subjects, was put through a three-day program with exactly the same procedure as that used for Group A except that on the third day, where Group A was given the strike-shock combination, Group F was given the shock alone. Two of the subjects in this group gave finger responses at the critical point.

Table 1 has been drawn up to permit a view of the experiment as a whole. Inspection of this table reveals the following points:

1. In those groups (B and D) where the buzz was coupled with no previous response, it evoked no finger reaction when tested at the critical point.

2. In Group E where the strike was first presented alone a number of times, then followed by a series of shocks, and again given alone, a finger response was evoked in one subject at the critical point. In Group F, where the procedure resembled that of Group E except that the buzz was conditioned to the strike and the buzz instead of the strike was tested, a response at the critical point was evoked in two subjects.

3. In those groups (A and C) where the buzz was coupled with *some* response and the shock which evoked the finger-withdrawal was coupled with *some* extra stimulus, responses were evoked at the critical point in 60 per cent and 70 per cent of the subjects, respectively.

Interpretation

As the experiment progressed, the employment of one additional control group after another was necessitated. It soon became apparent that, although differing in the specific problem with which each dealt, the present experiment and the former one concerned with "An Apparent Transfer of Conditioning" (9) were intimately related. Both were concerned with an analysis of the circumstances under which reactions that have been conditioned to given stimuli

TABLE 1

SHOWING THE SUCCESSIVE STEPS IN THE PROCEDURE OF THE EXPERIMENT, AND THE PERCENTAGE OF SUBJECTS RESPONDING AT THE TEST POINT FOR ALL SIX GROUPS

Group A (10 subjects, 3 sessions each)
1. Buzz and strike / Flash and strike } alternated
2. Flash and shock
3. Buzz → finger withdrawal (*60%* subjects)

Group B (10 subjects, 3 sessions each)
1. Strike / Flash and strike } alternated
2. Flash and shock
3. Buzz → finger withdrawal (*0%* subjects)

Group C (10 subjects, 3 sessions each)
1. Strike / Buzz and strike } alternated
2. Flash and shock
3. Buzz → finger withdrawal (*70%* subjects.)

Group D (10 subjects, 2 sessions each)
1. Buzz
2. Flash and shock
3. Buzz → finger withdrawal (*0%* subjects).

Group E (10 subjects, 2 sessions each)
1. Strike
2. Shock
3. Strike → finger withdrawal (*10%* subjects)

Group F (10 subjects, 3 sessions each)
1. Buzz and strike / Flash and strike } alternated
2. Shock
3. Buzz → finger withdrawal (*20%* subjects)

can become elicitable by other stimuli with which they have never been paired. Likewise the procedures involved in, and results obtained from, several of the control groups of the two experiments were closely comparable. In view of (1) their common objective, (2) the similarity of procedures employed, and, as will be brought out in the following discussion, (3) the interconsistency of their results, it seems advisable to consider the two experiments together in one interpretive discussion. They will be referred to as the former and latter experiments, respectively. The results of the former experiment have been reproduced and appear in Table 2.[6]

TABLE 2

SUMMARIZING THE RESULTS OF THE FORMER EXPERIMENT,* THE SUCCESSIVE STEPS IN THE PROCEDURE AND THE PERCENTAGE OF SUBJECTS RESPONDING AT THE TEST POINT ARE SHOWN FOR EACH OF THE THREE GROUPS

Group A	(15 subjects, 3 sessions)
1.	Flash and strike
2.	Strike and shock
3.	Flash → finger withdrawal (*60%* subjects)
Group B	(10 subjects, 3 sessions)
1.	Strike
2.	Strike and shock
3.	Flash → finger withdrawal (*0%* subjects)
Group C	(11 subjects, 3 sessions)
1.	Flash and strike
2.	Shock
3.	Flash → finger withdrawal (*9%* subjects)

*See reference (9).

By consulting Tables 1 and 2 it will be noticed that there were three groups in which a fairly high percentage of subjects responded at the critical point (60 per cent or more); another three in which a fairly low percentage responded (20 per cent or less); and, finally, three more in which none of the subjects responded. Three non-mutually exclusive hypotheses present themselves in tentative explanation of a portion of the facts. While for obvious reasons they must be considered as mere speculation, their presentation here seems justifiable in that they do tend to bring a certain amount of order to the facts. They may be termed (1) the hypothesis of response

[6]The major steps in the procedure are given in the table, which is comparable to Table 1. It will be noticed that in this experiment the flash (instead of the buzz) was used as the critical stimulus.

transfer between functionally equivalent stimuli; (2) the hypothesis of a generalization factor in the conditioning process; and (3) the conditioned-chain-reflex hypothesis. These will be discussed in the order given.

1. *The Hypothesis of Response Transfer between Functionally Equivalent Stimuli.* This hypothesis assumes that stimuli which have repeatedly occurred in conjunction with the same response tend to become functionally interchangeable so that at any time one may call out a response characteristic of another. Thus, if subsequently one such stimulus is connected with a new response, the others, because of their previous relation with this stimulus, will also tend to evoke this new response. The phenomena of *secondary extinction* and *secondary reinforcement*[7] point toward the likelihood of the existence of a functional relationship between two stimuli conditioned to the same response.

The present hypothesis demands responses to the critical stimulus in both Group A of the former experiment and Group A of the present one. In the former the critical stimulus, the flash, was repeatedly given together with the strike which produced the wink. Through being presented with the response of the strike, the flash would tend to become its functional equivalent and, after the strike had subsequently been conditioned to finger-withdrawal, would tend to function as a conditioned stimulus for this new response. In the latter group the flash and buzz were given alternately with the wink, and would likewise tend to become functionally equivalent through their contiguity with this common response. After the flash was reconditioned to finger-withdrawal, the buzz would also tend to function as a conditioned stimulus for this latter response.

This hypothesis, while it may help to explain the results of these two groups, does not account for the extremely positive ones of Group C of the former experiment, or for any of the slightly positive ones of the remaining groups.

2. *The Hypothesis of a Generalization Factor in the Conditioning Process.* Razran (8), in his discussion of "Dominance as Accounting for the Fundamentals of Conditioning," makes the point that

[7]See introduction and also references (6) and (10).

> Any biological theory of conditioning must first assume that there is some sort of connection, let it be subliminal, between any stimulus and any response, and that the differences among the various connections are only a matter of differences in some sort of thresholds . . .

and

> . . . the activation together of two patterns produces some specific lowering of the threshold between the stimulus of one and the response of the other pattern . . .

The present hypothesis makes both these suppositions, but in addition assumes that once a stimulus has become conditioned to a given response, its threshold for evoking other responses, particularly those which have been employed in conditioning combinations, tends likewise to become lowered.

In the experiments under consideration there were three groups —namely, Group A of the former experiment, and Groups A and C of the present one, in which a fairly high percentage of subjects (60 per cent or more) responded to the critical stimulus at the test point by withdrawing the finger. Two conditions were fulfilled in each of these groups; *both* the *stimulus* and the *response* in question had previously been components of conditioned-reflex combinations. In the remaining six groups, in which only one of these two conditions was fulfilled, the percentage of subjects responding at the test point was low (20 per cent or less). This seems to suggest a factor of generalization inherent in the conditioning process itself.

The results of other experiments on conditioned reflexes are also suggestive of such a generalization factor. Miller and Konorski (5) report that they observed, in a conditioning experiment on a dog, a type of phenomenon which they have called "motor generalization." The animal had been conditioned to raise its hind paw at the sounding of a piano note and to raise its front paw at the flashing of an electric light. During the establishment of these two conditioned reactions a recording device was always attached to the paw from which a reaction was expected, while the other paws remained free. After conditioning was established it was found that if the recording device was attached to either one of the two paws which had been involved in the experiment, the animal would re-

spond to either the note or flash by withdrawing the paw. On the other hand, when a device was attached to either of the paws which had not been involved in the experiment neither of these stimuli evoked any response whatsoever.

A type of generalization of conditioning, finding its expression in a negative way, has manifested itself in Pavlov's laboratory. In his discussion of "Conditioned Inhibition" (6) he points out that once a stimulus has been established as a conditioned inhibitor for a given conditioned reflex it will not only exert a strong inhibitory effect when presented with other homogeneous conditioned stimuli, but also when presented with heterogeneous conditioned ones. Experiments in point are cited, and he states:

> . . . it seems . . . that the experiments justify our conclusion that when a conditioned inhibitor has been firmly established the additional stimulus itself acquires inhibitory properties which can be manifested outside the parent combination. (p. 75 ff.)

The present hypothesis, supported by some additional experimental evidence, serves fairly adequately to set off the three groups showing a high degree of responsiveness from the remaining ones.

3. *The Conditioned-Chain-Reflex Hypothesis.* It will be recalled that the procedure in both experiments was so designed that all the respective groups were put through two fairly discrete steps, and that in several of these groups the stimulus employed in the first step finally came to evoke the response previously peculiar to the second step.

The object of the present discussion is to consider how this phenomenon might be attributed to a "chaining" of stimuli and responses, with the conditioning process serving to bind together the two respective links of the chain.[8] The problem at hand, therefore, is to show: (1) how such a chaining could account for the responses where they occurred; (2) how there could be no possi-

[8]An experiment is reported by Miller and Konorsky (4) in which what appears to have been a type of conditioned chain reaction was obtained in a dog. Here an extra stimulus was presented with the adequate one for the raising of the paw, which in turn was immediately followed by the presentation of food. After sufficient reinforcement it was found that the presentation of the extra stimulus alone caused a lifting of the paw which in turn was followed by what had been the response to the food.

bility of its operation in those groups which gave no responses at all; and (3) how it could account for the varying degrees of responsiveness of the different groups.

The hypothesis under consideration demands that the stimulus employed in the first step produce a response, either conditioned or unconditioned, and that the kinaesthesis from this response serve as the conditioned stimulus which in turn evokes the final and critical reaction.[9] It becomes evident (1) that there are three conditions which must be fulfilled if the chain conditioned reaction is to operate, and (2) that if these conditions are fulfilled, the reaction *must* tend to occur. These conditions are: (*a*) the conditioning of an additional stimulus to the critical response in the second step; (*b*) the elicitation of a response, either conditioned or unconditioned, by the critical stimulus employed in the first step; and (*c*) the identity of the kinaesthesis of the critical response in the first step with the additional or conditioned stimulus of the second step.

A consideration will be given to the various mechanisms by which the first two of these conditions might have been fulfilled in the various groups. This will be followed by an examination to determine in which of the groups the third condition was also fulfilled.

Condition A. With respect to the fulfillment of this condition, there exists in all the groups of both experiments the possibility of what for the lack of a ready term may be called "co-response" conditioning. By this is meant the conditioning which may occur between two responses contiguously elicited by the same stimulus. To be more specific, if it is granted that the shock tended to evoke a wink as well as withdrawal of the finger (which, no doubt, frequently did occur) the kinaesthesis of the wink would immediately follow the adequate stimulus for this withdrawal, i.e., the shock, and backward conditioning between the two should take place. Likewise, a similar backward conditioning between the kinaesthesis of the finger-withdrawal and the lid reaction would be expected. This latter possibility, however, has no direct bearing on the present results. Since all the groups in both experiments were given the shock in the second step of the procedure, it might be expected in the light of this mechanism of "co-response" conditioning that an excitatory

[9] Such a hypothetical mechanism has been presented diagrammatically by Hull (3).

tendency between the kinaesthesis of the wink and withdrawal of the finger would be set up. This possibility will be referred to as *mechanism 1.* The conditioning occurring in this way would probably be rather weak, however, because of its backward direction. [It will be recalled that Wolfle (11, 12) as well as Ebbinghaus (2) has shown this to be less effective than the forward direction for the establishment of excitatory tendencies.]

Such a "co-response" conditioning mechanism would demand at the test point (1) that the lid reaction be present, and (2) that it slightly precede the withdrawing of the finger. By using a more sensitive recording device for the lid movements and a more rapidly moving recording surface, both these points could be determined.[10]

In Groups A, B, and C of the present experiment and Group B of the former one, two additional excitatory tendencies might also be expected to develop. In these groups, where an additional stimulus was presented approximately 1/3 second before the incidence of the shock, this additional stimulus should have become conditioned to both the wink and the withdrawal of the finger which occurred in response to the shock. But because of the tendency of conditioned reactions to creep forward temporally toward the conditioned stimulus, it is within possibility that the conditioned wink and finger-withdrawal were brought forward sufficiently actually to precede the incidence of the shock. In this event the two reactions (wink and finger-withdrawal) would occur twice; once in response to the conditioned stimulus, and again in response to the shock. But in addition to the "co-response" conditioning of the reaction to the shock, there would be a second "co-response" conditioning taking place in the reaction to the conditioned stimulus (the flash). This excitatory tendency, which necessarily would be relatively weak because of (1) the backward order of the conditioning and (2) the feebleness of the responses, will be referred to as *mechanism 2.* But an additional stronger mechanism would also be expected to operate here—namely, the forward conditioning between the kinaesthesis of the conditioned wink (to the flash) and finger-withdrawal from the shock. This will be referred to as *mechanism 3.*

It is realized, of course, that the hypothesizing of these two mech-

[10]The Dodge Pendulum-Photochronograph (1) would serve both these purposes excellently.

anisms is attended by certain theoretical complications. In the first place, if the conditioned finger response to the flash were sufficiently great the finger would be completely removed from the electrode and no shock would be received; in this event there would be a single instead of a double response. In the second place, the conditioned response to the flash might be in a refractory phase at the incidence of the shock so that the responses to the latter would tend to be inhibited. In the third place, double responses of both the finger and the eyelid should have appeared on the record where they actually failed to do so. Perhaps, however, under more refined recording techniques these double responses might have shown up.

A very critical experiment in point would be to put two additional groups through exactly the same procedure as Group C (or A) except for a change in the temporal relations between the flash and the shock. Wolfle's experiments on conditioned finger-withdrawal (11, 12) showed the percentage of subjects becoming conditioned to be practically identical (1) when a forward-conditioning interval of 1.00 second was employed, and (2) when simultaneity of stimulation by the conditioned and unconditioned stimuli was employed. If, then, two groups in which these two intervals were employed (1.00 second and simultaneity) were put through the otherwise same procedure as Group C (or A), the following results would be demanded by the present hypothesis:

1) In the group in which the forward interval of 1.00 second was employed the forward-creeping tendency of the conditioned reaction to the flash should be sufficient to produce a perceptible double response on the record, one to the flash and a second to the shock.

2) This same group should show an appreciably greater percentage of responses at the crucial point because of the optimal conditions for the operation of mechanisms 2 and 3. In the other group there would be no opportunity for the operation of these mechanisms.

On the other hand, if the supposed mechanisms were not operative one would expect (1) no double responses, and (2) no appreciable difference in the percentage of conditioning in the two groups.

In Groups A and B of the former experiment, there exists, in

addition to mechanisms 1, 2, and 3, a fourth possible excitatory tendency which also might contribute toward the fulfillment of condition A. This is the forward-conditioning tendency between the kinaesthesis of the unconditioned wink to the strike and withdrawal of the finger from the shock. This will be referred to as *mechanism 4.* Because of (1) the relatively great intensity of the conditioned stimulus (the kinaesthesis of the unconditioned wink) and (2) the forward direction of the conditioning, this excitatory tendency should be comparatively strong.

It is fully realized that the occurrence of the unconditioned wink to the strike, though slightly preceding, would nearly coincide with the supposed anticipatory wink and finger-withdrawal upon which mechanisms 2 and 3 depend. Whether or not under these conditions the two responses would tend to inhibit each other, enhance each other, or combine, is difficult to say. But by using more sensitive and rapid recording techniques these points could be somewhat cleared up. Under such conditions it would be possible to determine whether or not these two responses actually did occur. Then by subsequently extinguishing the anticipatory conditioned response by discontinuation of the shock it should be possible to determine the effect which the conditioned reaction had been exerting on the unconditioned one; if the extinction of the conditioned response was accompanied by an augmentation of the unconditioned one it would seem probable that prior to extinction the former had been inhibiting the latter. Conversely, if the extinction of the conditioned response was accompanied by a diminution of the unconditioned one it would seem probable that prior to extinction the former had been augmenting the latter. Finally, by comparing the magnitude of these anticipatory conditioned reactions with those of groups similar to A or C of the present experiment it would be possible to determine whether the unconditioned wink exerted an inhibitory or facilitory effect on the anticipatory conditioned one.

Condition B. With respect to this condition—namely, the elicitation of a response by the stimulus employed in the first step of the procedure—the number of possibilities by which fulfillment could be attained is fairly limited. In the present experiments there are but two: (1) the critical stimulus may have been capable of

eliciting an unconditioned response from the beginning; or, (2) though without a response in the beginning, it may, during the course of the step, have acquired a conditioned one by being presented a number of times in temporal contiguity with it.

In Group B of the former experiment and Groups B and D of the present one *this condition simply remained unfulfilled.* In these groups the stimulus employed (either flash or buzz) evoked no overt unconditioned response, and, being presented with no other response, it was given no opportunity to acquire one.

In Group E of the present experiment the stimulus employed (the strike) was presented with no additional response, so that in this step it was given no opportunity to acquire a conditioned one. But from the beginning it did produce an unconditioned response in its own right, which is sufficient for the fulfillment of this condition.

In the remaining groups, A and C of the former experiment and A, C, and F of the present one, the critical stimulus employed in this step (flash or buzz) evoked no overt response in the beginning. But by being accompanied by the strike, which evoked the wink, the critical stimulus was given an opportunity to acquire a conditioned wink. Furthermore, since in all these groups approximately 1/3 second elapsed between the incidence of the critical stimulus and that of the strike, the resulting conditioning would be expected to be fairly strong.

Condition C. The fulfillment of this condition—namely, the identity of the kinaesthesis of the response in the first step with the additional or conditioned stimulus of the second step—obviously depends on the fulfillment of the other two conditions. Consequently, those groups in which neither of the other conditions were met need not be considered here. Altogether there were six groups, A and C of the former experiment and A, C, E, and F of the present one, in which both these conditions were fulfilled to some degree. In all these groups the critical stimulus supposedly was capable of evoking a response by the end of the first step, and in every case this response was a *wink.* In the second step of the procedure of these same groups the critical reaction was always accompanied by, and therefore given an opportunity to become conditioned to, the *kinaesthesis*

TABLE 3

SUMMARY OF THE EXPERIMENTAL DATA IN THEIR RELATION TO THE CONDITIONED-CHAINED-REFLEX HYPOTHESIS*

Group	Type of response elicited by the critical stimulus of the first step (Condition *B*).	Possible mechanisms by which excitatory tendencies could have become established in the second step of the procedure (Condition *A*)	Identity of kinaesthesis of response in *B* with stimulus in *A* (Condition *C*)	Percentage of subjects responding at crucial point
1-A	conditioned	1, 2, 3, 4	yes	60
1-B	none	1, 2, 3, 4	no	0
1-C	conditioned	1	yes	9
2-A	conditioned	1, 2, 3	yes	60
2-B	none	1, 2, 3	no	0
2-C	conditioned	1, 2, 3	yes	70
2-D	none	1, 2, 3	no	0
2-E	reflex	1	yes	10
2-F	conditioned	1	yes	20

*Numbers in first column refer to experiment. 1 indicates the previous experiment (9); 2 indicates the present one. Letters refer to subgroups.

of a wink. It therefore follows that in all six of these groups this condition, as well as the other two, was fulfilled, and in all of them the hypothesis would demand responses at the crucial point.

The discussion of this hypothesis has been rather lengthy and involved. Table 3 has been constructed to bring together its salient points. This table shows with respect to each group (1) the ways in which each of the requisite conditions for the conditioned chain reaction could have been met; and (2) the percentages of subjects responding at the crucial points.

It will be observed that there are three groups—namely, Group B of the former experiment and Groups B and D of the present one, in which there was no possibility of condition *C* being met. Therefore, on the basis of the hypothesis, no responses at the critical point would be called for in these groups. This is exactly in accordance with the facts; none of these groups gave any responses.

In each of the remaining groups there existed the possibility of fulfillment of all three of these requisite conditions. The hypothesis would consequently demand at least some positive responses in each. Again the hypothesis accords with the facts; each of these remaining groups did respond to some extent.

But not only does this hypothesis serve to differentiate between the positive and negative groups; it also serves to account for the difference between the strongly and weakly responding ones. In Group A of the former experiment and Groups A and C of the present one, those in which there existed the possibility of three or more mechanisms operating toward the establishment of conditioning between the kinaesthesis of the wink and withdrawal of the finger, the percentage of responses was very much greater than in the remaining groups where but one of these mechanisms could have been operating.

Summary

Two groups of ten subjects each were put through an experimental program consisting of two discrete conditioning steps. The first step consisted in the repeated pairing of extra stimuli with a sudden rap on the cheek which served to evoke a wink. In one group a buzz and flash were used as extra stimuli; in the other the buzz alone was used. The second step consisted for both groups

in the repeated pairing of an extra stimulus (flash) with a shock which served to evoke withdrawal of the finger. At the end of the experimental program the extra stimulus of the first step (buzz), which had never been given in the second step, was presented alone and found to evoke finger-withdrawals in an appreciable percentage of the subjects of each group. These percentages were 60 and 70, respectively.

The employment of another group of ten subjects revealed that finger-withdrawal did not occur at the test point unless the test stimulus was one that had been previously used in one of the two steps of the experiment. Finally, the employment of still another group of ten subjects revealed that finger-withdrawal occurred in but a small percentage of subjects (20 per cent) when the shock employed in the second step had not been paired with an extra stimulus.

Thus this reaction did not occur with any appreciable degree of regularity except when the test or critical stimulus had been coupled with some response and the critical response had been coupled with some extra stimulus. (The results of a previous related experiment stand in essential agreement.) This seems to indicate that the reaction depends on the previous conditioning of both the test stimulus and the critical response. It seems, therefore, that what is here being dealt with is some kind of *indirect conditioned reaction.*

Three tentative hypotheses have been put forward in an attempt to order the experimentally obtained facts. These are: (1) the hypothesis of response transfer between functionally equivalent stimuli; (2) the hypothesis of a generalization factor in the conditioning process; and (3) the conditioned-chain-reflex hypothesis.

This investigation must be considered solely as exploratory, and the hypotheses put forward as purely speculative. Any adequate explanation or interpretation of the results must await further experimentation. However, from these results the general conclusion may be drawn that stimuli which have acquired conditioned responses may under certain circumstances call out, directly or indirectly, other conditioned responses with which they have never been associated.

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LE CONDITIONNEMENT INDIRECT

(Résumé)

Deux groupes, chacun composé de dix sujets, ont subi un programme expérimental composé de deux étapes conditionnelles différentes. La première étape s'est composée de la mise répétée en paires de stimuli supplémentaires avec un coup soudain sur la joue lequel a eu le but de causer un clin d'oeil. Pour un groupe on a employé un son bourdonnant et un jet de lumière comme stimuli supplémentaires; pour l'autre on n'a employé que le son. La seconde étape pour les deux groupes s'est composée de la mise répétée en paires d'un stimulus supplémentaire (jet de lumière) avec un choc qui a causé le retirement du doigt. A la fin du programme expérimental on a présenté seul le stimulus supplémentaire de la première étape (son bourdonnant) qu'on n'a jamais présenté dans la seconde étape, et ce stimulus a causé des retirements des doigts dans le cas de 60 et de 70 pour cent des sujets, respectivement.

Les groupes de contrôle ont montré que la réaction ne se montre pas avec un degré de régularité sauf quand l'on a joint le stimulus du test ou stimulus critique à quelque réponse et la réponse critique à quelque stimulus supplémentaire.

Shipley

INDIREKTES BEDINGEN

(Referat)

Zwei Gruppen von je zehn Vpn. machten ein experimentelles Programm durch, das aus zwei getrennten, bedingenden Stufen bestand. Die erste Stufe bestand aus der wiederholten Paarung von Extrareizen mit einem plötzlichen Schlag auf die Wange, der ein Blinzeln hervorrief. In einer Gruppe wurden ein Burren und ein Lichtstrahl als Extrareize verwandt; in der anderen wurde das Burren allein gebraucht. Die zweite Stufe bestand bei beiden Gruppen in der wiederholten Paarung von einem Extrareiz (Licht) mit einem elektrischen Schlag, der die Zurückziehung der Finger verursachte. Am Ende des experimentellen Programms wurde der Extrareiz der ersten Stufe (Burren), der in der zweiten Stufe nie gegeben war, allein dargeboten, und es steilte sich heraus, dass er Fingerzurückziehungen bei 69 bzw. 70 Prozent der Vpn. verursachte.

Kontrollgruppen zeigten, dass die Reaktion nicht mit irgendeinem Grad von Regelmässigkeit vorkommt, ausser wenn der Test oder kritischer Reiz mit irgendeiner Antwort und wenn die kritische Antwort mit irgendeinem Extrareiz verbunden wurde.

SHIPLEY

THE PITCH SPECIFICITY OF AUDITORY ADAPTATION*

From the Psychological Laboratory of Brown University

C. H. Pearce

Introduction

It has been shown in several investigations that if the human ear is stimulated for some seconds or minutes by a relatively pure tone the judged loudness of the tone will decrease, as indicated by a change in the lower loudness limen, or by reference to a comparison tone presented to the other, relatively unstimulated ear. Studies of various aspects of this phenomenon, which has been called adaptation or fatigue, have led to somewhat conflicting conclusions.

Reports vary in regard to the effect of different intensities of the continued tone. Békésy (2) is of the opinion that intensity has little relation to the amount of adaptation. Pattie (5) shows that adaptation is greater with certain increases in intensity, but that the relation is not a linear one. Achmatov (1) concludes that variations in intensity are of marked importance for the degree and duration of the adaptation effect.

No consistent relation has been found to hold between the amount of adaptation and the duration of the adapting tone, although in general longer stimulus duration is followed by longer and stronger adaptation. In most cases sensitivity is said to return gradually after cessation of the adapting tone.

Comparatively little work has been done to determine to what extent adaptation is confined to the pitch producing it. Of such studies, that of Weinberg and Allen may be mentioned first (7). These investigators find that the frequency with which a tone must be interrupted in order that a just continuous sensation may be obtained is lowered by previous stimulation for two minutes by a tone of that pitch. In the range of frequencies between 142 and 285

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cycles the effect is less if the adapting tone is of slightly higher or lower pitch. If the prolonged tone differs in pitch from the tone measured by more than 15 cycles no adaptation occurs. Since the adaptation is confined to this small band of frequencies the results are interpreted by Weinberg and Allen as support for a resonance theory of audition.

In a general investigation of auditory fatigue Pattie tested the specificity of fatigue as a function of pitch. The work reported in the present paper includes a modification of Pattie's technique, therefore the latter will be outlined in some detail. The relatively pure tone generated by a Western Electric 2-A Audiometer was presented to both ears by headphones. One ear was fatigued while the other ear was stimulated only occasionally to make comparison judgments. The two tones were made nearly equal in loudness, and fatigue was indicated by the relative loudness increase of the tone presented to the fresh ear, as compared with the original loudness ratio between the two tones. This ratio was determined by so fixing intensity in the comparison tone that the latter was judged softer in a majority of cases. To obtain fatigue the other ear was stimulated for one minute. Upon discontinuance of this tone the original pair of tones was given, one at each ear in succession. Judgment was required as to whether the tone presented secondly, in the comparison phone, was "louder," "equal," or "softer." This procedure was repeated at ten-second intervals for 100 seconds. If the continuously stimulated ear was rendered less sensitive the comparison tone was reported louder more often than at first. The frequency of such "louder" judgments was determined by repeating the fatigue period with its subsequent testing pairs for 15 or 20 times. In the total number of judgments for the respective ten-second intervals the decreasing number of "louder" judgments as time passed after the beginning of recovery indicated increasing sensitivity. Duration of the fatigue effect was measured by the time necessary for the number of "louder" judgments to return to the normal percentage. The whole recovery process could be represented on a graph, of which the abscissae signified time in seconds after cessation of the fatiguing stimulus, and the ordinates showed percentages of "louder" judgments.

In order to determine whether fatiguing at a certain pitch had

more effect upon the loudness of that tone than upon others of different pitch, Pattie obtained in each sitting two groups of results; in the first the fatiguing tone and the test pair were of the same pitch; in the second the fatiguing tone was of different pitch, but of the same loudness as measured by the number of sensation units above the lower loudness threshold. For example, in the test group he presented a fatiguing tone of 512 cycles, and then gave the standard intensity ratio with the same pitch. In the other group the fatiguing tone was 1024 cycles, with the same two intensities, at 512 cycles, being given after interruption of the fatiguing tone. In this way the amount and duration of the fatigue at 512 cycles when the fatiguing stimulus was at that pitch was compared with the effect on the same tone when the fatiguing tone was at 1024 cycles. In 10 such sittings the comparison fatiguing tone was at 550 cycles in five cases, 627 in three cases, and 1024 and 1330 cycles in one case each. In these sittings the amount of fatigue (degree and duration) at the higher pitch, as compared with that at 512 cycles, was the same in five cases, more in two, and less in three. Pattie concludes that fatigue set up with one tone is present in all others used.

In another group of sittings Pattie modified the above procedure by omitting the series in which the fatiguing tone was 512 cycles and comparing the amount of fatigue directly with the loudness ratio established at first. If the fatigue at a given pitch did not appear in other pitches there would presumably be no curve of recovery—only an irregular line approaching the horizontal. In all of 15 sittings with this method there were recovery curves indicating the presence of some fatigue.

Békésy, also with a binaural procedure, has reported somewhat similar results. He concludes that exposure to a given tone causes a temporary reduction of sensitivity to tones over a broad range of frequencies on either side of the fatiguing tone. The adaptation effect is maximum at the frequency of the prolonged tone and decreases rapidly with distance from that frequency. A curve representing these relations shows that fatiguing with a tone of 800 cycles affects tones negligibly beyond 500 cycles and 2000 cycles, but with greater effect upon the lower tones than upon the higher.

In the three investigations reviewed, the major difference appears

between the results of Weinberg and Allen on the one hand, and those of Pattie and Békésy on the other. This may be due to one or both of two facts. The first study involved one ear; the others two. Second, the criterion of fusion may have a different basis than that of decrease in loudness. Both Pattie and Békésy agree that adaptation is not specific to the tone which sets it up.

The present study undertakes a more elaborate examination of the pitch specificity of auditory adaptation.

Experimental Conditions

Western Electric headphones were separately wired from a General Radio Beat Frequency Oscillator, model 613-A, series 13. An attenuator connected with both phones allowed intensity to be varied in steps of two decibels. A variable resistance introduced into the circuit of the phone to the right ear varied intensity through a range including and on either side of the fixed intensity in the left phone.

The room used was quite free from sound, although not absolutely sound proof. Rubber cushions enclosed the phone and covered the ear, making for a comfortable and sound-resisting joint. The subject's back was turned to the apparatus so that he could not see the adjustments.

The method of measuring adaptation was, with several modifications, that of Pattie. The stimulus duration was constant at 45 seconds throughout. With such short stimulus periods recovery was rapid; only four judgments were taken—at 0, 10, 20, and 30 seconds after cessation of the continuous tone. No equal judgments were permitted; if the tones appeared indiscriminable the subjects were advised to guess. After the training period of one or two sittings little difficulty was reported in making decisions. Four women and four men, all with some training in psychology, served as subjects.

In an experiment such as this it is to be expected that judgments will be affected by judgments preceding them in the series, and consistent reports will be associated with particular circumstances, such as the adapting pitch. These factors were kept at a minimum by insertion of irrelevant comparison pairs, which were not considered in the final totals. It was implied by the procedure that the intensity ratio was constantly being altered. Care also was taken to keep

the manner of opening and closing the key constant, and to allow a constant time between adaptation intervals sufficient to complete recovery.

Both methods employed by Pattie were used; namely, that using one, and that using two adapting pitches. It was found that the percentage of "louder" judgments taken at the start of the session did not substantially agree with that of judgments taken at the end. The fifty judgments were therefore obtained in four or five groups throughout the sitting, thus affording a more comparable basis for judgments subsequent to adaptation throughout the one-hour sitting.

In order that the adapting tones might be comparable with the test tones save in pitch they were taken at the same number of decibels above the lower loudness limen. Usually there were fifteen judgments taken alternately with the two adapting pitches. In this way the conditions for obtaining adaptation were nearly constant for the two pitches.

Method 1 was that in which the normal percentage line, or base line, served as an indicator of the presence or absence of adaptation.

Method 2 consisted in direct comparison of the adaptation set up in a tone when the adapting tone was of the same and when it was of different pitch. The curve of returning sensitivity could still be studied with reference to the base line, and this method had the advantage of indicating whether the amount of adaptation was small or great for the different pitch.

Method 3 simply introduced a third adapting pitch, with the same test pitch being used as standard for measuring adaptation at its own pitch and at two others. We shall call these adapting tones respectively the test pitch, A-1 and A-2. This method had the advantage of yielding the same results as the other two methods, and in addition that of comparing the effects of two tones of different pitch from the test pitch upon that pitch.

Experimental Results

Table 1 summarizes the results of 28 sittings with methods 1 and 2. In each case the tones used were an equal number of decibels above the lower threshold, between 80 and 100 decibels in all cases. Successive columns refer to subject, number of judgments, normal percentage of "louder" judgments, test pitch, percentage of "louder"

TABLE 1
SUMMARY OF SITTINGS INVOLVING TWO DIFFERENT PITCHES

S	J's	Nor. %	Test p.	0	10	20	30	Ad. p.	0	10	20	30	a	b
E	17	20	2000	94	82	60	60	3000	59	30	35	35	M	M
G	15	33	2000	87	80	53	60	1600	53	53	33	33	M	M
P	15	35	2000	87	71	53	60	1600	47	47	27	20	M	M
A	15	22	1600	60	26	33	20	2500	33	13	20	20	S	M
E	17	38	1200					1600	82	71	45	41	M	
C	16	25	1200					1600	63	60	44	19	M	
C	15	48	1200	93	100	80	80	1800	86	47	80	60	M	M
P	15	38	1200	87	81	60	20	1800	47	40	67	47	S	M
A	16	44	1200	88	60	63	30	1800	69	50	60	60	M	M
C	16	46	2500					1600	88	63	60	44	M	
A	19	2	1600	100	56	21	11	1400	84	37	16	16	M	M
C	15	2	1600	86	53	33	33	1400	86	60	53	33	M	S
A	18	6	1400	100	62	39	62	1600	41	33	28	16	M	M
S	15	4	1400	93	38	19	19	1600	100	33	19	19	M	S
P	20	47	500	85	75	70	80	2048	65	65	40	40	M	M
G	15	33	2000	87	80	53	60	512	67	67	47	33	M	M
A	15	4	2500					512	5	5	5	0	S	
A	20	4	400	53	30	5	0	480	55	20	10	0	M	S
E	22	34	512					1600	65	50	41	41	M	
E	20	20	512					2500	25	20	20	25	S	
E	17	36	2500					512	47	77	47	65	M	
S	15	8	400	80	0	7	7	480	84	14	0	7	M	S
C	15	20	800	93	60	47	47	256	71	60	40	27	M	M
C	18	2	400	50	25	19	13	480	78	40	28	11	M	L
C	16	25	1200					1400	60	27	27	27	M	
R	20	22	512	100	85	80	60	2000	80	70	50	35	M	M
S	14	40	500					600	93	64	51	36	M	
S	14	40	500					1200	43	14	34	14	S	

judgments after adaptation at the test pitch, second adapting pitch, and its recovery percentages. *a* indicates whether the number of "louder" reports is more, less than, or the same as the normal percentage. *b* indicates whether the total number of "louder" judgments at the test pitch is more, less than, or equal to that at the test pitch.

Table 2 summarizes for method 3 the same results given in Table 1 for methods 1 and 2, including 25 sittings. The last two columns, marked *c* and *d*, will be considered presently.

The following remarks apply to both tables. Pitches used varied from 256 cycles to 3000 cycles. In four cases (Table 1) two pitches were separated by 80 cycles (400, 480), and in one case by 100 cycles (500, 600). In all other cases the difference in frequency ranged between 450 and 1500 cycles. In method 3 the two adapting pitches were selected at comparable distances from the test tone. Six sittings not represented show that with method 2, stimulation at 500 and 512 cycles yielded a total difference of 3 to 4 "louder" judgments out of about 70 comparisons; stimulation at 1400 and 1410 cycles gave a difference of about 4 to 6 "louder" judgments after adaptation.

An analysis of Tables 1 and 2 with respect to *a* and *b* elicits the following facts. In 66 out of 78 cases in which the effect of stimulating with a certain pitch upon another pitch is considered, there is some evidence of adaptation as indicated by method 1. In the 12 exceptions the difference in pitch between the adapting and test tones amounts to 600 cycles or more. However, although there is ordinarily some spread of effect, the degree of adaptation as indicated by *b* amounts to that produced by the test tone itself in only 6 out of 48 sittings. In the 6 exceptions the difference in pitch between the test and comparison tones is 200 cycles or less in 4 instances. In other words, if the tones differ by more than 200 cycles the adaptation is greater if the adapting and test tones are of the same pitch.

As thus stated the results summarized under *a* and *b* are in contrast with the report of Weinberg and Allen. The present results confirm Pattie's indication of a lack of sharp specificity of adaptation, but do show that there is more consistent localization of adaptation than his results would suggest.

It is evident that with variations from sitting to sitting in thresh-

TABLE 2
SUMMARY OF SITTINGS INVOLVING THREE DIFFERENT PITCHES

S	J's	N. %	Test p.	0	10	20	30	A-1, A-2	0	10	20	30	a	b	c	d
P	15	35	800	93	67	67	60	256	60	60	47	33	M	M	V	O
								1400	60	60	33	27	M	M		
	15	23	2000	87	71	60	53	512	67	54	47	33	M	M	W	P
								1600	47	47	27	20	M	M		
	15	30	800	100	100	80	53	256	53	67	47	40	M	M	V	P
								1400	20	40	27	27	S	M		
	15	43	2000	100	93	93	71	512	80	74	67	47	M	M	W	P
								3000	73	67	47	33	M	M		
	20	20	800					256	40	30	30	25	M		V	O
								1400	35	30	15	15	M			
R	15	40	800	100	100	93	60	256	71	67	33	47	M	M	V	P
								1400	47	47	20	33	S	M		
	15	18	1600					256	100	87	60	47	M		Y	P
								800	100	87	67	40	M			
	15	37	2000	93	71	87	53	512	73	51	40	33	M	M	W	P
								3000	47	27	33	33	S	M		
	15	27	2000	100	100	67	40	512	87	67	40	20	M	M	W	P
								3000	71	53	27	20	M	M		
	15	20	800	93	60	47	47	250	71	60	40	27	M	M	V	P
								1400	40	33	33	13	M	M		
G	18	9	512					1600	11	11	11	6	S		Y	P
								2500	22	6	11	6	S			
	15	30	800	93	80	80	47	256	87	60	47	33	M	M	V	P
								1400	60	33	40	33	M	M		
	15	33	2000	87	80	53	60	512	67	67	47	33	M	M	W	P
								1600	53	53	33	33	M	M		
	15	30	2000	100	74	37	27	800	53	40	27	20	M	M	W	P
								1600	47	53	27	27	M	M		

TABLE 2 (*Continued*)

S	J's	N.%	Test p.	0	10	20	30	A-1, A-2	0	10	20	30	a	b	c	d
S	17	17	600					1600	31	13	6	13	S		Z	O
								2500	95	18	12	12	M			
	16	19	512					1600	94	31	13	13	M		Z	P
								2500	88	38	19	19	M			
C	17	33	512					1600	65	47	17	11	M		Z	P
								2500	65	53	35	11	M			
	20	10	2000					1500	70	30	30	5	M		X	P
								2500	65	40	20	5	M			
W	16	22	700	100	69	63	19	256	94	75	69	31	M	S	V	P
								1600	69	50	25	19	M	M		
	15	22	1800	93	100	60	47	1200	73	20	40	33	M	M	X	P
								2500	60	43	40	20	M	M		
	20	45	1600					256	70	55	60	60	M		Y	O
								800	85	75	70	60	M			
	15	20	1600					250	67	60	53	40	M		Y	P
								800	60	60	67	53	M			
	16	20	2000	100	75	69	38	1400	94	56	25	19	M	M	X	P
								3000	85	56	38	25	M	M		
	17	18	1600	100	90	54	30	700	96	60	54	48	M	M	W	P
								2500	90	42	30	6	M	M		
	20	13	2500					700	90	65	25	20	M		W	P
								1600	60	30	10	10	M			

olds, bodily condition, and normal loudness ratio, the results obtained with particular pitches are too few to allow summing of percentages for the purpose, for example, of drawing a curve to represent spread of adaptation. Therefore summarizing will be limited to remarks about particular sittings. This is done in Table 2, where the adaptation set up in one tone by each of two others is considered. If for convenience we call tones of 1200 cycles or more high tones, and those of 800 or under low tones, we can in each sitting in Table 2 compare the effects of high or low tones, or both, upon another high or low tone. When the table is analyzed in this way the results are grouped in five sets.

Category *V* includes the cases in which the effects of a high and a low tone upon a low tone are compared. In 5 out of 7 sittings the lower tone has the greater effect.

Category *W* includes the cases in which the effects of a high and a low tone on a high tone are compared. In 7 out of 8 sittings the lower tone has a greater effect.

Category *X* includes those cases in which the effects of two different high tones upon another high tone are compared. In 3 cases the effects of the two tones are nearly equal.

Category *Y* includes the cases in which the effects of two low tones on a high tone are compared. In 3 out of 4 sittings the effect of the two low tones is about the same.

Category *Z* includes the cases in which the effects of two high tones upon a low tone are compared. In 2 out of 3 sittings the effects are about equal for the two high tones.

In the above results the exceptions are usually equivocal rather than entirely opposed to the major trend. In general it seems safe to conclude that as a rule tones of less than 1000 cycles tend to have more adaptation effect on tones of other pitches, both below and above 1000, than do pitches above 1000 cycles. In Table 2 heading *c* refers to the category, and *d* indicates whether the results are positive (P) or doubtful (O) for the above generalization. It is pertinent to note that, out of 12 instances in which there was apparently no adaptation, only once was the adapting pitch below 1000 cycles.

In order to test some of the above results, a different method was used. This consisted in rendering a very faint tone inaudible

by stimulating with another loud tone, and observing the time necessary for the first tone to be heard again. The method was not adopted because it presented fewer difficulties to accurate judgment, but for the reason that it employed but one ear. Table 3

TABLE 3

SUMMARY OF SITTINGS INVOLVING THREE DIFFERENT PITCHES; LOWER THRESHOLD METHOD

Sub.	J's	A-1	Av. sec	Test	Av.	A-2	Av.	c
G	11	1500	2.8	2000	8.2	2500	2.6	X
	15	256	7.9	700	10.3	1600	1.3	V
	12	256	11.3	1500	13.5	800	12.2	Y
	15	700	13.2	1600		2500	2.3	W
C	11	256	8.1	700	7.8	1600	2.8	V
	14	256	5.5	700	7.1	1600	1.1	V
	20	700	12.5	2500		1600	4.9	W
	15	700	12.4	2500		1600	3.8	W
W	15	256	7.5	700	8.3	1600	1.1	V
	14	256	7.2	700	5.9	1600	1.4	V
	15	1600	2.1	2500	10.1	3500	1.3	X

contains the results obtained with the monaural technique. The average time in seconds for return of the threshold tone is indicated for the three cases (or two) in which the pitch of the stimulus tone is that of the faint tone, or of two other pitches. Heading *c* refers to the categories of Table 2. Tables 2 and 3 give similar facts about the two methods, save that in the latter, average recovery time is substituted for percentage of "louder" judgments. The intensity level of the tone stimulating for 45 seconds was the same as that in the previous cases.

All of the 11 sittings with the lower-threshold method yield results which conform to the generalization made for Table 2; namely, that low pitched tones occasion more adaptation than do high pitched ones.

The amount of adaptation undergone by a tone after stimulation by a high or low tone may now be restated. Out of 49 cases in which a high tone was used as a prolonged stimulus 11 showed no evident adaptation at another pitch, while in 29 cases in which a low tone was used only one case failed to show spread of adaptation to another pitch. The difference in adaptation following stimulation

by high or low tones is further indicated by the observation that in 23 cases in which the amounts of adaptation following stimulation by a high and a low tone were compared, 20 cases showed greater effect for the low tone. In 13 sittings in which both stimulus tones were either high or low, 11 showed the effects to be nearly equal.

In the curve offered by Békésy, mentioned above, the adaptation following stimulation by a tone of 800 cycles may be compared at specific points with the results obtained in Tables 2 and 3. In that curve an 800-cycle tone evidently has the same effect upon a 1500-cycle tone as upon a 500-cycle tone. With the same stimulus tone here, as noted, a tone of 256 cycles showed more adaptation than one of 1400 cycles.

Summary and Conclusions

The experimental results of this study may be summarized as follows.

1. If relatively pure tones of presumably comparable intensity are allowed to stimulate the ear for 45 seconds, subsequent tones of pitches between 256 and 3000 cycles may appear less loud than previously.

2. If the prolonged tone is 1200 cycles or more in frequency there are not infrequent exceptions to generalization 1.

3. If the prolonged tone is 800 cycles or less such exceptions as mentioned in 2 are rare.

4. As a rule the loudness decrease in a tone is more if the continued tone is of the same pitch rather than of a different one.

5. Adaptation following prolonged stimulation by lower pitched tones is greater than that following higher pitched tones.

If the reliability of these conclusions is accepted, the question of the relation they bear to theories of audition may be raised. The results seem to imply a difference in the amount of adaptation due to lower or higher frequencies. If it is assumed that the adaptation is localized in the vibrating masses of the cochlea, results upon stimulating with tones of lower pitch fit in better with a "time" explanation of pitch reception, and those upon stimulating with higher tones fit in better with a "space" explanation, as the terms time and space are ordinarily used in this connection. The results are most in harmony with the supposition that both temporal and

spatial factors cooperate in all pitch reception, with one or the other factor predominating, as indicated.

Fletcher (4) has given a detailed exposition of a "space-time pattern theory of hearing." Others, for example Troland (6), have suggested such a possibility. Davis and Saul (3) have stated that musical notes presented at the ear of the operated cat result in action currents which are most clearly recovered from the auditory tracts between the frequencies of 200 and 1000 cycles. They offer the tentative opinion that 1000 cycles is the highest frequency which the auditory path can transmit.

The emphasis of the present writer would be more strongly upon a refinement and validation of the techniques used and suggested in the work reported here than upon rearing prematurely a theoretical superstructure upon generalizations expressing the data collected in this study. Despite its limitations the approach to auditory mechanics by way of adaptation or fatigue appears to afford definite possibilities.

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LA SPÉCIFICITÉ DE HAUTEUR DE L'ADAPTATION AUDITIVE

(Résumé)

Chez huit sujets le changement dans l'intensité d'un ton après la stimulation par un ton de la même hauteur ou d'une hauteur différente a été étudié pour 78 paires de tons, en employant l'intensité dans l'autre oreille comme étalon. Les résultats suivent:

1. Quand le ton d'un oscillateur de fréquence des battements stimule l'oreille pendant 45 secondes, les tons subséquents d'une intensité presque égale entre 256 et 3000 cycles peuvent paraître moins intenses qu'auparavant.
2. Si le ton prolongé a une fréquence de 1200 cycles ou plus, il n'y a aucune diminution de l'intensité du second ton en 11 sur 41 cas.
3. Quand le ton prolongé est de 800 cycles ou moins, il y a une diminution de l'intensité dans 36 sur 37 cas.
4. La diminution de l'intensité d'un ton est plus grande si le ton prolongé précédent est de la même hauteur.
5. L'adaptation suivant une stimulation prolongée par des tons d'une plus petite hauteur est plus grande que celle suivant des tons d'une grande hauteur. Cette généralisation a été confirmée dans 11 séances où l'on a employé une méthode monaurale d'un plus bas seuil.

PEARCE

TONHÖHENSPEZIFIZITÄT DER GEHÖRANPASSUNG

(Referat)

Bei acht Versuchspersonen wurde die Änderung des Lautschalls eines Tones, die auf die Reizung mit einem Ton derselben oder verschiedener Tonhöhe folgte, für 78 Paare von Tönen bei der Verwendung des Lautschalls in dem anderen Ohr als Massstab untersucht. Folgende sind die Ergebnisse:

1. Wenn der Ton von einem Schlag-Häufigkeitsschwingungsapparat (beat-frequency oscillator) das Ohr für 45 Sekunden reizt können nachfolgende Töne von beinahe gleicher Intensität zwischen 256 und 3000 Zyklen weniger laut als vorher erscheinen.
2. Wenn der verlängerte Ton von 1200 Zyklen oder mehr an Häufigkeit ist, gibt es keine Abnahme des Lautes des zweiten Tones in 11 von 41 Fällen.
3. Wenn der verlängerte Ton von 800 Zyklen oder weniger ist, gibt es eine Abnahme des Lautes in 36 von 37 Fällen.
4. Die Abnahme des Tonschalls ist mehr, wenn der vorangehende, verlängerte Ton derselben Tonhöhe ist.
5. Die Anpassung nach verlängerter Reizung mit Tönen niedriger Tonhöhe ist grösser als die nach Tönen einer hohen tonhöhe. Diese Verallgemeinerung wurde in 11 Versuchen bestätigt, in denen einen einohrige, niedrige Schwellenmethode verwandt wurde.

PEARCE

DIFFERENTIAL SENSITIVITY AS DETERMINED BY AMOUNT OF OLFACTORY SUBSTANCE*

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As regards differential sensitivity in olfaction, psychophysics has three investigations to its credit. Gamble (5) secured evidence which seemed (in 1898) to support Weber's law as applied to smell; Toulouse and Vaschide's results (16) agree closely with those of Gamble; and the statistical constants of Hermanides (7) indicate that the Weber ratio is approximately constant for a weak and a strong olfactory substance of the same chemical structure. The chief interest in each of these studies, however, was centered not in sensitivity *per se* but in the applicability or "verification" of Weber's law in regard to smell. There appears to be no experimental account in the psychophysical literature on smell of the way in which differential sensitivity varies as a continuous function of some measurable aspect of the stimulus. To remedy this want is the object of the present paper.

METHOD

In order to determine the functional relationship between differential sensitivity and different amounts of a given olfactory substance, six points were first selected on the olfactory stimulus-continuum. At each of these points, sensitivity to the olfactory substance was estimated by psychophysical procedure for dirhinic presentation. The psychophysical method used is that which has been recently christened the *method of single stimuli* (13). It is formally defined as "any psychophysical method in which a report follows the presentation of one stimulus only" (13).

Schumann (11) called attention to the fact that, in presenting successive stimuli for comparison, judgment often is made before the second member of the pair is presented. His view received experimental support in the well-known work of Martin and Müller

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on lifted weights (8). Since then, the comparative validity and apparent usefulness of the method of single stimuli has been tested and justified several times (4, 10, 18). For psychophysical work in smell, the method is singularly appropriate: it avoids the errors attendant upon simultaneous, and (often) immediately successive, presentation of two odors. Thus, it affords greater accuracy as well as greater economy of time and effort.

Apparatus and Procedure[1]

The apparatus, in general, was a form of simple olfactometer (19, pp. 85-86; 20, pp. 85-86) adapted to the requirements of the problem. As such it provided for stimulations over a wider range of intensities than has heretofore been used for psychophysical experiments in smell (5, pp. 110-111; 20, p. 167 and p. 252).

Two osmoscopes—one for each subject—were made of glass tubing 0.8 cm. I.D. (2, pp. 259-260). Either one could be readily inserted into the outlet of the exposure-tube, secured by narrow strips of surgeon's tape, and sealed with tinfoil. Both osmoscopes were bent upwards 90° at the ends to accommodate the nostrils (5, p. 110). The osmoscopes were constructed according to separate specifications for each subject by the Macalaster Bicknell Company, Cambridge, Massachusetts.

At a point about 8 cm. from the union of osmoscope and exposure-tube, the tube passed through a small hole (2.3 cm. diam.) in an upright screen (56 x 72 cm.) which was erected at right angles to the axis of the apparatus in order to prevent the subject from witnessing the setting of the stimuli during experimentation. The outer glass tubing was graduated conveniently to indicate the amounts of olfactory substance exposed. The points at which sensitivity to olfactory intensity was tested were: 10, 50, 100, 200, 300 and 400 olfacties. The five stimulus-amounts that were used as variables in determining psychometric functions at each of the six points were also read from the graduated scale.

For purposes of direct comparison, it would seem desirable to take equal separations between the variables at all six points. This, how-

[1]This experiment was performed, in the main, in Amherst during the summer of 1934. Acknowledgment is due Professor H. N. Glick, of the psychological laboratory at the Massachusetts State College, for many courtesies and special consideration.

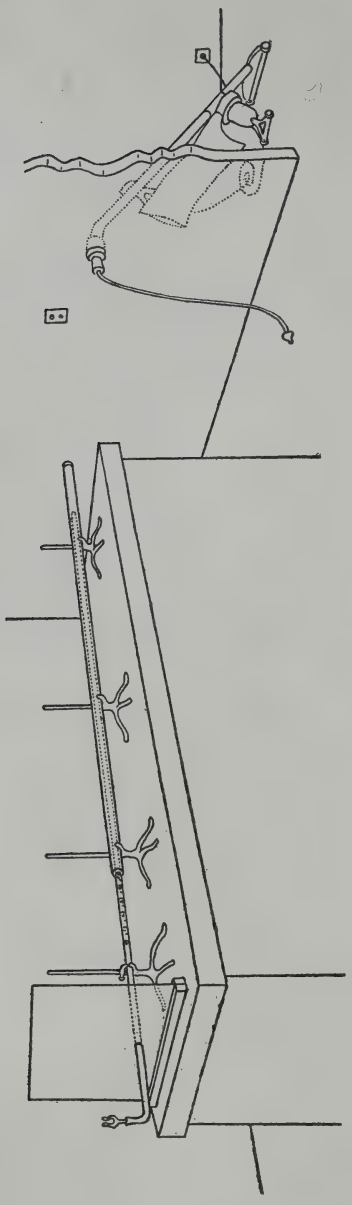

FIGURE 1

APPARATUS USED FOR OLFACTOMETRIC PRESENTATION

ever, is impossible in practice. Variable separations at the six points on the stimulus-continuum were therefore unlike, although separations between the five variables at any given point for a particular series were the same. The size of step between variables at each point was selected empirically on the basis of promise to yield a satisfactory psychometric function, *i.e.,* (1) no inversions, (2) the smallest p not greater than .25 but more than .00 and (3) the largest p not less than .75 but not so great as 1.00.

Stimulations were made through the exposure-tube, a glass tube 340 cm. long and 2.3 cm. in diameter. The glass tube containing the olfactory material was slipped over the exposure-tube, which was firmly secured in a horizontal position by felt-lined clamps. The olfactory substance was measured in terms of amount of material exposed. Areal unit values were transmuted into conventional ("physiological") olfactory units by means of the standard of measurement in smell—the *olfactie* (20, pp. 118-122). It denotes the smallest amount of a given olfactory substance that a person with normal sensitivity can perceive when presentation is made in a simple olfactometer.

Only two categories were employed: "strong" and "weak." Each of the subjects (the writers) was set to judge the intensity of the presentation relative to the absolute series. One hundred reports for a given stimulus-series determined the psychometric function for each of the six points. The variables of each series were always presented in a random order.

There are at least two courses open to the investigator who wishes to study the manner in which olfactory sensitivity varies with change in amount of stimulation: (1) by the use of a single olfactory substance, or (2) by the use of a number of substances. The more usual procedure is to employ a number of different substances. Although the latter procedure presents certain advantages, it is nevertheless possible that the way in which changes in stimulus-amount mediate variations in the psychophysical process may become the more clearly apparent if the work be limited to a thoroughgoing investigation *via* a single appropriate material. To this end, all observations were restricted to a single olfactory substance: India rubber.

India rubber was selected because it affords a distinctive odor, is manufactured in a form convenient for experimental purposes,

and is highly recommended for *psychophysical* experiments in smell. According to Gamble, for instance,

> India rubber has three great qualifications for use in experiments in smell. (1) It can be smelled for a long time by most subjects without blunting the organ; (2) its odor is not easily obscured by other odors, and (3) adheres comparatively little to the inhaling-tube (5, p. 113; *cf.* also 15, p. 132).

Zwaardemaker recommends the use of India rubber on quite similar grounds (20, pp. 92-93). Furthermore, psychophysical experiments which are concerned with the problem of olfactory sensitivity are best performed with a substance that does not change the index-response in kind when different amounts of the substance are presented under a special attitude. A number of odors are known to manifest qualitative modulation (*Geruchswechsel*) when different amounts, or concentrations, of a given substance are employed (6, pp. 180-186; 13, pp. 253-255). India rubber, however, does not belong to this group: likeness of olfactory quality is positively predominating when widely differing amounts of this olfactory material are used. Incidentally, it may be observed that it was in terms of India rubber that the olfactie was first determined. For this substance, an olfactie equals 1.76[2] (20, p. 87 and p. 118).

The subjects were trained to inhale in a standard manner. Deep inhalation is said to be unsatisfactory for refined olfactory discriminations, and "sniffing" is usually disallowed in well-controlled experiments in smell (5, p. 103). The mode of inhalation was standardized as follows. At the "ready" signal, the subject exhaled through the mouth, closed the mouth, adjusted the nostrils to the osmoscope, and inhaled gently for a period of five seconds (stop-watch). Each of these operations was performed carefully and in as nearly as possible a like manner at every trial. The osmoscope was always adjusted into the anterior part of the *external nares* which Fick long ago stated to be the optimal region for smell discrimination (15, p. 118). Attempt was also made to insert the end of the tube into the nostril to the same depth and to keep the pressure of the nostril against the osmoscope the same in all trials. To prevent the subject from accidentally breathing back into the osmoscope between ob-

[2]The "original" *Kautschuk Riech-Messer* is described in (19, pp. 89-90).

servations, the position of the apparatus was a trifle high relative to the nostrils (5, p. 102). Between trials the subject was permitted to relax in his chair, and when next signalled to observe, he responded by a forward and upward movement in adjusting to the osmoscope.

During continued stimulation by a smell-substance, the olfactory and other excited receptors undergo adaptation (9, pp. 363-364; 20, pp. 165-177). These receptors also undergo adaptation-changes when an odorous material is presented intermittently during a restricted period of time. In order to reduce the probable occurrence of functional changes which thus result from frequently recurrent presentations of the same odor, "rest" periods were given. After every twenty-five reports, a five-minute period of relaxation was allowed. Rest periods were also introduced whenever the subject reported continued uncertainty in judging, whenever he asked outright for a rest, and whenever the experimenter observed by inspection that the recorded replies were markedly inconsistent.

In order that the measurement might fall within the limits of conventional accuracy, the following precautions were additionally observed. The olfactory substance was housed in a sealed case to prevent escape of the rubber particles from other regions than on the inner surface of the stimulus material. When not in use, the exposure-tube was completely sealed in order to preserve the freshness of the rubber.[3] The experimental room was kept relatively free from other olfactory materials which might combine with the olfactory particles proceeding from the substance to the receptor. The air in the olfactometer was "deodorized" immediately preceding each trial. This was effected mechanically by the use of a ventilating unit, an accessory to the apparatus. When a presentation was to be made, the experimenter adjusted the olfactometer to the desired extent, turned on the switch to the electric motor of the ventilating unit (an Air-Way Electric Cleaner) and adjusted the connection between the ventilating unit and the end of the apparatus that was

[3]Since the rate of evaporation varies with changes in temperature, it is desirable that the room temperature remain fairly constant (5, p. 94; 11, p. 121). The average temperature readings taken from a thermometer in the experimental room on the days when regular experiments were performed was 24° C (±3°).

remote to the subject. In this way, the exposure-tube was cleansed before every stimulation.[4]

The period of time between exposure of stimulus and the subject's verbal report was maintained approximately constant. Trials were limited initially by the moment when the experimenter adjusted the olfactometer to present a given variable and finally by that at which the subject rendered his (forced) judgment; it included (*a*) setting of olfactometer by the experimenter, (*b*) ventilating the apparatus, (*c*) turning off electric motor ("ready" signal), (*d*) the subject's adjustment to the osmoscope, and (*e*) the five-second inhalation which terminated in the verbal report.

Results

Table 1 contains the values necessary to a psychophysical description of the process studied under the conditions of this experiment. The table presents for each subject the mean of each stimulus-series, the range employed at each point, the relative frequencies of the "strong" replies in increasing order of stimulus-strength, and (in the last three columns) the values of the statistically determined constants. The ΔR of the table stands for the difference between the stimulus-values corresponding to $p = .84$ and $p = .50$ on the various psychometric functions. It is determined (approximately) by computing the root-mean-square deviation of the differential curve derived from the hypothesis (*phi-gamma*) representing the probabilities of the "strong" replies. The values of $\Delta R/R$ are listed in the final column. The Weber-fraction apparently decreases with an increase in stimulus-amount within the limits of 10-400 olfacties.

The relationships existing between the values of h and the stimulus amount, for both subjects, may be perceived with greatest economy optically. Figure 2 illustrates the general manner in which h, the precision-index, varies as a function of the number of olfacties presented. This value, h, is sometimes taken to stand directly for the differential sensitivity of the receptive mechanism under investigation.[5] Since the subjects were "set" (1) by instruction and (2)

[4]Acknowledgment is gratefully made to Mrs. T. G. Alper, Miss L. W. Gates, and Mr. L. M. Hurvich for assisting in the capacity of experimenter.

[5]*Cf.* Fechner (3, 48 f.); see also Thomson (13), Culler (1) and Wever and Zener (14, pp. 485 f.). The respective recommendations are h, 2/Q, 1/PE, and (again) h. Without exception, and under the assumptions involved in the *phi-gamma* hypothesis, these measures are either equal or directly proportional to one another.

TABLE 1

PSYCHOMETRIC FUNCTIONS OF INTENSIVE DIFFERENCES FOR OLFACTOMETRIC PRESENTATIONS OF INDIA RUBBER. RELATIVE FREQUENCIES BASED ON 100 REPLIES FOR EACH STIMULUS-SERIES

R Mean value of stimulus-series (Olfacties)	Subjects	Range of five equally separated stimulus-values (Olfacties)	Relative frequencies of "strong" replies: successive stimuli in increasing order of olfactometric strength					h	ΔR	$\frac{\Delta R}{R}$
			1	2	3	4	5			
10	A.H.H.	0- 20	20	30	40	70	85	.0718	9.85	.985
	M.J.Z.	0- 20	15	30	50	75	85	.0780	9.067	.9067
50	A.H.H.	30- 70	20	25	45	70	85	.0334	21.38	.4236
	M.J.Z.	22- 78	20	40	50	70	90	.0249	28.40	.568
100	A.H.H.	60-140	10	20	55	65	80	.0192	37.20	.372
	M.J.Z.	44-156	15	30	55	70	85	.0125	56.58	.5658
200	A.H.H.	160-240	20	40	45	60	80	.0135	52.91	.2645
	M.J.Z.	140-260	15	35	50	60	80	.0104	68.00	.34
300	A.H.H.	240-360	25	35	55	70	95	.0110	64.93	.2164
	M.J.Z.	240-360	25	40	55	65	75	.0080	88.40	.2947
400	A.H.H.	320-480	15	35	45	80	90	.0103	69.95	.1749
	M.J.Z.	320-480	25	30	55	75	90	.0090	78.58	.1964

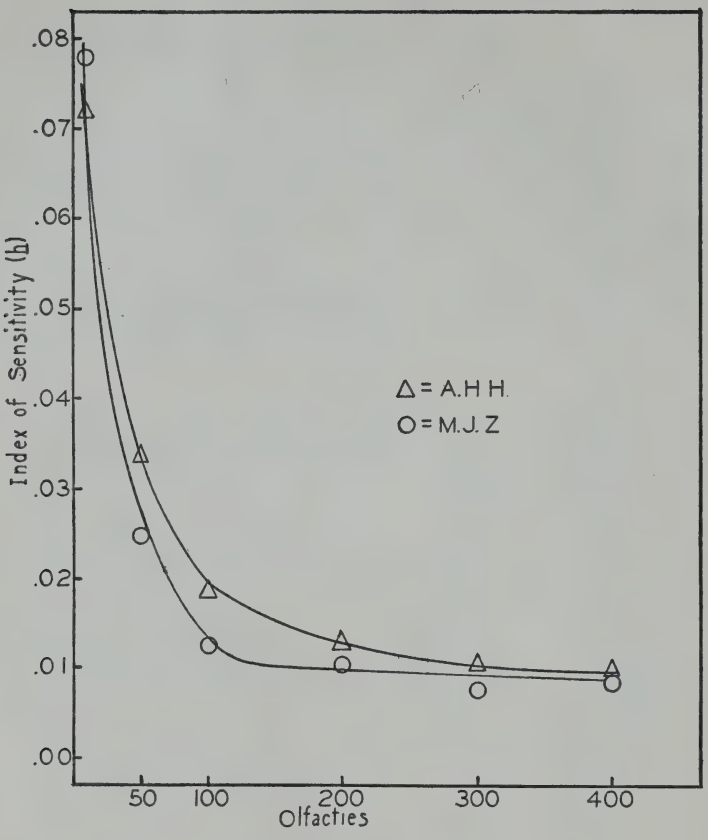

FIGURE 2

by training to discriminate with respect to intensity, differential sensitivity here has reference to the *total* intensity produced by the India rubber particles in a state effective for excitation and for a presentation which lasts five seconds. In this context,

$$h = k \frac{dI}{dR} = aR^{-b}$$

where h is a measure of differential sensitivity, I is taken to be the intensity of the olfactory process, R is the magnitude of the stimulus in olfacties, and a and b are constants. Differential sensitivity to

India rubber varies directly with the ratio of the total intensity to the magnitude of the stimulus, and is in form a hyperbolic function of the stimulus (*cf.* Figure 2).

Summary

Differential sensitivity to India rubber was tested by the method of single stimuli at six points on the olfactometric stimulus-continuum between the limits of 10 and 400 olfacties. The precision-indices (h) were determined by standard psychophysical procedure (Urban) for the six points. The manner in which differential sensitivity for two subjects is functionally related to the strength of the olfactory substance is illustrated graphically. The graph indicates that sensitivity and olfactometric strength are probably related by a function that is hyperbolic in form.

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LA SENSITIVITÉ DIFFÉRENTIELLE DÉTERMINÉE PAR LA QUANTITÉ DE SUBSTANCE OLFACTIVE

(Résumé)

On a testé la sensitivité différentielle au caoutchouc au moyen de la méthode des stimuli seuls à six points sur le stimulus-continuum olfactométrique entre les limites de 10 et 400 olfacties. Les indices de précision (h) ont été déterminés par le processus ordinaire psycho-physique (Urban) pour les six points. On illustre graphiquement la manière de laquelle la sensitivité différentielle pour deux sujets se rapporte fonctionnellement à la force de la substance olfactive. Le graphique indique que la sensitivité et la force olfactométrique se rapportent probablement l'une à l'autre par une fonction qui est hyperbolique en forme.

ZIGLER ET HOLWAY

DIE BESTIMMUNG DER DIFFERENTIALEMPFINDLICHKEIT DURCH DIE MENGE DER GERUCHSSUBSTANZ

(Referat)

Die Differentialempfindlichkeit gegen Kautschuk wurde mittels der Methode des einzelnen Reizes auf sechs Punkten des geruchsmetrischen Reizkontinuums zwischen den Grenzen von 10 und 400 Geruchswerten untersucht. Die Präzisionsindexe (h) wurden mittels des normalen psychophysischen Verfahrens (Urban) für die sechs Punkte bestimmt. Die Weise, in der die Differentialempfindlichkeit sich für die zwei Vpn. funktionell auf die Stärke der Geruchssubstanz bezieht, wurde graphisch aufgezeichnet. Die Aufzeichnung zeigt, dass die Empfindlichkeit und geruchsmetrische Stärke wahrscheinlich durch eine Funktion verbunden sind, welche hyperbolisch in Form ist.

ZIGLER UND HOLWAY

THE RELATION OF INSTRUCTIONS TO ELECTRICAL RESISTANCE AND PERFORMANCE*

From the Department of Psychology, University of Kentucky

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The frequent use, on the part of psychologists, of the concept of set has led to some suspicion that it is doing duty for the abandoned notion of instincts. Differences in learning, in observation, and in emotions among individuals and within the same individual are frequently explained in terms of set. It seems to the writer that often such explanations are inadequate without some measure of sets. An investigation of the influence of set upon the various aspects of mental life may aid in understanding the relations existing between these aspects. "Sets," like instincts, have been variously defined. In this paper, we use the term as referring to the bodily adjustments of the organism to the task to be performed, and the task in which he is engaged, and we are assuming that the tension of the muscles plays a part in these adjustments.

Three kinds of facts led to the experiments reported below. First, it is a common statement in applied psychology (4) that the organism adjusts to the expected difficulty of the task. Thus, when an individual is told to grip the dynamometer as hard as he is able for ten times he will not squeeze it as hard upon the first attempt as he will when told to grip it only one time. Second, experimental findings have repeatedly indicated that more learning or mental work occurs when the organism is tense than when it is relaxed. Third, several experiments have indicated that muscular relaxation brings with it an increase in apparent electrical resistance, and that mental work and muscular contractions are accompanied by a decrease (10).

In the light of these facts several problems arise. Will measurements of electrical resistance reveal a difference in muscular tension according to the expected difficulty of the task? Will there be a difference between alleged mental work and alleged physical work? Will directions indicating the relative difficulty of the task, the

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complexity of which is unknown to the subject, affect differentially the electrical resistance? Finally, in the event that directions do influence the electrical resistance of the organism, will the amount of learning or mental work which follows be related in any manner? Since it has been found that the apparent electrical resistance varies with muscular tenseness and that within limits the more tense the organism the greater the learning, we would expect to find some relationship.

Experiment I

To determine whether measurements of apparent electrical resistance would differentiate between sets we performed an experiment in which we induced sets which are generally recognized as different (3). Eight subjects, taken individually, were attached by a leather cuff to a recording galvanometer in the circuit of which was a wheatstone bridge. In the subject's circuit was placed .9 volt of alternating current. The apparent ohmic resistance of the subject was recorded on a moving roll of paper every 5 seconds by a pen attached indirectly to the galvanometer. The paper was ruled horizontally in 250-ohm units and vertically in 2½-minute intervals. The paper, governed by a synchronous motor, moved 6 inches an hour. Zinc electrodes were attached to the dorsal and palmar surfaces of the subject's left hand. A paste of kaolin, physiological saline, and zinc sulphate was spread on one surface of the zinc plate. Separating the paste from the subject's hand was a layer of absorbent cotton moistened with physiological saline. To prevent drying of the electrodes, sponges moistened with water were placed around the electrodes.

The subject was seated in a morris chair on the right arm of which was attached a Smedley hand dynamometer. The position of the dynamometer on the arm of the chair and the adjustment of the stirrup varied from subject to subject. The position was such that the subject could grip the dynamometer without extending his arm, which was flexed at the elbow and rested upon the arm of the chair. The subject was brought into the experimental room, the electrodes attached, the dynamometer adjusted, and a card, on the under side of which were the directions, was placed on the right arm of the chair. The subject was then told that he was to rest

for approximately 15 minutes, and that at the end of the rest period a buzzer would sound which would be the signal to turn the card and read the directions. To acquaint the subject with the buzzer, it was immediately sounded. The experimenter went into the room which contained the apparatus, closing the door between the two rooms. The galvanometer was turned on and, after exactly 15 minutes, the signal to read the directions was given. The rest period was for the purpose of obtaining a relatively stable level of electrical resistance and to permit the subject to become adapted to the situation.

There were two kinds of instructions given. One card (I) read:

> The next time the buzzer sounds you are to squeeze the dynamometer with your right hand, as hard as you are able, without raising your arm. You are to continue squeezing it as long as the buzzer sounds. You will be asked to squeeze the dynamometer only one time.

The other card (II) was like the first except that for the last sentence was substituted, "The buzzer will sound 10 times and you are to squeeze the dynamometer each time." The fact that the above instructions elicit different sets is clearly indicated by Morgan (3).

Each subject was given both cards, but the order of presentation was alternated. The signal to grip the dynamometer was given 2½ minutes after the signal to read the card of directions. There was a 10-minute rest interval between the two conditions to allow the subject to recover from any fatigue he might have experienced. The fatigue could not have been great, for the buzzer sounded for five seconds only, and even following Card II the dynamometer was gripped only once. In other words, the instructions to grip the dynamometer ten times were not carried out. This failure, in the case of those individuals who received Card II first, might have affected the performance on the other set of instructions, but no such effect was noted either in the electrical resistance curves or in the amount of pressure exerted on the dynamometer.

Following the first task, the experimenter switched off the motor operating the recording apparatus, but not the current in the subject's circuit; went into the subject's room, read the dynamometer,

which was placed so that the subject could not read it; removed the card of instructions; placed the other card in the same position; and informed the subject that in about 10 minutes the buzzer would indicate that he was to read the second card.

The results in terms of mean electrical resistance appear in Table 1. In the column headed "Signal I" is the mean resistance of

TABLE 1

RESULTS IN TERMS OF APPARENT MEAN ELECTRICAL RESISTANCE

Instructions	Signal I	M. E.	Signal II	M. E.	Difference	Percentage difference
I	8600	993.85	7445	923.39	1155	13.4
II	8165	1281.43	6178	1044.78	2087	25.5
Difference	435		1267		932	12.1

the 8 subjects immediately before the buzzer sounded for the subject to read the instructions. In the column headed "Signal II" is the mean resistance of the subjects just prior to the signal to grip the dynamometer. Roman numerals I and II refer to cards I and II, respectively.

While the number of cases is small, it appears that measurements of electrical resistance will differentiate the adjustments which the organism makes to the two tasks. The mean difference in electrical resistance between the two groups of subjects recorded under Signal I was 435 ohms; 2½ minutes later, the difference was 1267 ohms or nearly three times as large. The drop in electrical resistance following Card II is 12.1% greater than the drop following Card I.

Since our primary interest is whether measurements of electrical resistance are related to the set of the organism, some measure of this relationship within the individual might be more informative than the mean changes given in Table 1. For each subject the difference in resistance between Signals I and II was converted into the percentage of resistance under Signal I. The percentage so obtained for each subject in Series II was subtracted from the percentage so obtained in Series I. The reading of the dynamometer obtained in Series II was subtracted from the reading of the dynamometer obtained in Series I; the relation of this differ-

ence to the reading in Series I was stated in terms of percentage. Seven of the 8 subjects exerted more kilograms of pressure in that series in which they had the greater percentage decrease in electrical resistance. However, only 5 of the 8 exerted their maximum pressure in Series I. The mean superiority of these 5 was 7.5 kilograms. One subject gripped the dynamometer equally hard in the two series; the other two exerted a mean pressure of 4.5 kilograms more in Series II than I.

From the above considerations, we may conclude that in this experiment (1) changes of electrical resistance prior to the performance of the physical task were related to the efficiency with which the task was performed, (2) the kind of instructions given was also related to the performance of the task, but not as closely as changes in electrical resistance, and (3) a drop in electrical resistance during the interval between instructions and performance was followed by a greater performance than a rise in resistance during the same interval.

Experiment II

Since we obtained a difference in electrical resistance in situations which are generally recognized to have different sets (3,4), the same technique might be used in investigating sets not so clearly distinguished. The experimental literature on learning has been equivocal with reference to the influence of encouragement, discouragement, instructions that a task is difficult or easy, and similar differences in incentives. It may be that a clue to this disagreement will be found in an objective study of the relation of instructions to the preparatory sets in the learning situation. With this in mind, the following experiment was conducted.

Three alphabetical codes were placed on discs for the Ranschburg exposure apparatus. One of these was the Morse code; another a code similar in construction to the Morse but made of triangles; the third was similar but made of circles. Reading from left to right in the exposure window there first appeared the letter of the alphabet and then the code equivalent.

When each of the 27 subjects came into the room the electrodes were attached and, after 15 minutes, general instructions were given. These instructions were as follows:

> This is an experiment in learning. The material which you will be asked to learn consists of code equivalents of the alphabet. Each letter with its equivalent in code will be shown you separately on the disc. Five repetitions will be made. After this you will be given a sheet of paper with the alphabet typed on it on which you are to record the code equivalents of the letters. Please do not talk during the experiment.

In addition to the general instructions three types of directions labeled for convenience "hard," "normal," and "easy" were used.

> *Hard*: This code uses —— and —— as symbols. Naturally it will be difficult for you to learn the code. I expect you to do as well as possible, but the difficulty of the code will undoubtedly prevent you from recalling many of the equivalents. Now we shall begin.
>
> *Easy*: This time we have a code which is so easy that it may seem rather silly for me to ask you to learn it. —— and —— are the symbols used in this code. It is really surprising how easy it is to learn a large portion of this code. Do your best. Now we shall begin.
>
> *Normal*: This code will be presented to you in the manner which we stated before. Do your best and try to learn all of the equivalents that you can. The symbols are —— and ——. Now we shall begin.

The counterbalanced order was used with codes and directions in order to control at least partially the influence of previous instructions upon subsequent ones and changes in the learning process that might occur by virtue of having learned a particular previous code.

The galvanic hook-up used in this experiment is one suggested by Darrow (2) and described in a previous experimental article reported from the University of Kentucky laboratory (10). The electrodes were the same as those used in the hook-up described above. The reasons for substituting the Darrow hook-up were (1) a more accurate control of the current passing through the subject and (2) a more sensitive arrangement.

The apparatus used in this experiment, with the exception of the exposure apparatus, was placed in a room adjoining the subject's room. The metronome, adjusted to make 30 electrical con-

nections per minute, operated the exposure apparatus. The metronome was placed in a relatively sound-proof box in the apparatus room.

The procedure with the subject was as follows: He was seated so that his arms could rest comfortably on a table on which, directly in front of him, was placed the exposure apparatus. The electrodes were attached to the palmar and dorsal surfaces of his left hand. One of the experimenters, after attaching the electrodes, went into the apparatus room, connected the galvanometer, and started an interval timer. The other experimenter[1] remained in the subject's room and after the interval timer had indicated the elapse of 15 minutes, during which the subject was not disturbed, read him the general instructions. Half a minute after the general instructions one of the types of special instructions was read to the subject. Five presentations of the code followed immediately. After the presentations, the subject was given the recall sheet. He was allowed 3 minutes to write the equivalent code letters. He was then told that he would not be asked to recall the particular code again and was given a 3-minute rest period. The second type of special instructions was given, followed by the second code. The procedure was the same for the third as for the other two codes.

The experimenter in the apparatus room recorded resistance readings at the following points: beginning of rest period; after 5, 10, and 15 minutes of rest; during the reading of the general instructions; during the reading of the special instructions; at the beginning of the presentation of each code and at the conclusion of the presentations; during recall; and three times during the rest interval.

To determine whether hard and easy directions are followed by differences in electrical resistance, the individual resistance recorded at the end of the long rest interval was divided into the subject's resistance recorded immediately following the presentation of the various types of instructions and these values converted into percentages. A mean of the percentages so obtained for all subjects was determined for each of the three types of instructions. Table

[1]The writer wishes to thank Miss Dorothy Clifton for her assistance in this experiment.

TABLE 2

VALUES GIVEN IN TERMS OF PERCENTAGE OF THE AVERAGE MEAN RESISTANCE

	1 Hard	2 S. D.	3 Easy	4 S. D.	5 Ob. D. (1&3)	6 S. E. D.	7 Nor-mal	8 S. D.	9 Ob. D. (1&7)	10 S. E. D. (1&7)	11 Ob. D. (3&7)	12 S. E. D. (3&7)
1	85.5	23.0	99.6	21.1	14.1	6.5	101.1	30.3	15.6	8.1	1.5	7.8
2	86.5	18.1	92.8	22.5	6.3	3.1	*					

*Normal instructions were not given in this experiment.

2, row 1, gives these means together with the standard deviation of the distribution and the standard error of the differences between any two of the averages.[2]

The chances are 98 out of a hundred that the difference obtained between the electrical resistances following hard and easy directions is a true one. An experimental verification would perhaps be more valid since only 27 subjects were used. Such verification is presented in row 2 of Table 2. These averages were obtained in another experiment having for its purpose an investigation of a different phase of the problem; the procedure was the same except that ideational material was auditorially presented, instead of visual presentation of codes, and no "normal" instructions were given. The results from 18 subjects are available. The people who served as subjects in this experiment were not the same as those who served in the preceding experiment. The chances are 99 out of 100 that there is a difference in the average electrical resistance following hard and easy directions.

Information concerning the number of subjects whose records are consistent with the means appearing in Table 2 aids in the evaluation of the data. An examination of the data upon which row 1 Table 2 is based shows that 22 out of the 27 subjects had higher resistance following presentation of the easy directions than following hard directions. Only 3 subjects had as low resistance following easy directions as the average for the group following hard directions.

Are the above differences in electrical resistance related to the

[2]In a few instances in Tables 2, 3, and 4 it was necessary to eliminate two or three subjects because of apparatus difficulty.

number of code equivalents reproduced? Since a lowered electrical resistance is found in a comparatively tense organism and since tonicity and amount learned have been found to be related we should expect to find differences in reproduction related to differences in electrical resistance. Table 3 gives the mean number of code equivalents recalled under the three conditions.

TABLE 3

RESULTS IN TERMS OF CODE EQUIVALENTS RECALLED

1 Hard	2 S. D.	3 Easy	4 S. D.	5 Ob. Dif.	6 S. E. D.	7 Normal	8 S. D.	9 Ob. Dif.	10 S. E. D.
								(3&7)	(3&7)
7.54	3.73	5.56	3.11	1.98	1.09	6.60	3.10	1.04	.92
								(1&7)	(1&7)
								.94	1.00

From this table we see that the chances are 97 out of a 100 that the recall following hard instructions will be greater than that following easy directions. The chances that the obtained differences between recall following easy and normal directions (columns 3 and 7), and that following hard and normal directions (columns 1 and 7) are significant are 86 out of 100 and 82 out of 100, respectively.

The preceding results indicate a probable relationship between instructions, recall, and changes in electrical resistance. If one of the reasons for the superior recall following hard directions is the greater decrease in electrical resistance following these instructions a correlation between recall and the percentage decrease in electrical resistance ought to exist. Since the order of presentation of the codes varied from subject to subject and the total number of subjects was only 27 we cannot relate the amount of the code recalled to the change in resistance in any one series. In other words, we cannot correlate the amount recalled of the first, second, or third code presented with changes in electrical resistance. The first code presented to 9 subjects was the Morse, to 9 others, the circle code, and to the remaining 9, the triangle code. We have no reason to assume that the codes are of equal difficulty. It seems reasonable, however, to add the number of code equivalents reproduced in all

TABLE 4

CORRELATIONS OBTAINED BY THE USE OF SPEARMAN'S RANK-DIFFERENCE FORMULA

Total amount of the three codes recalled correlated with	R
1. Average resistance following directions in terms of percentage of original rest resistance	—.06
2. Average learning resistance in terms of percentage of original rest resistance	—.16
3. Average learning resistance	.33
4. Average increase of rest resistance over average learning resistance	.27
5. Average rest resistance in terms of percentage of original rest resistance	.04
6. Average rest resistance	.50

three series by each subject and to compare these totals with the changes in electrical resistance. Table 4 contains several coefficients of correlation between total recall and measurements of electrical resistance under the various conditions of our experiment. All coefficients were calculated through the use of the Spearman rank-difference formula. In the use of the formula rank number 1 was assigned to the largest number of code-equivalents recalled and to the largest percentage.

The fact that the correlation between rest resistance and learning when the former is calculated in terms of percentage change is zero and that the corresponding coefficient between learning resistance and amount recalled is —.16 fits in to some extent with the theory that the superior learner is likely to be more tense than the poor learner. The fact that the relationship between total recall and rest resistance is .50 and that the relationship between total recall and learning resistance is only .27 tends to support a similar explanation, i.e., the superior learners are likely to have greater absolute rises in electrical resistance during rest periods than poorer learners, due to greater relaxation. The fact that there was a marked tendency for the electrical resistance to rise during rest agrees with Wechsler's (8, p. 76) results on the relation of mental work to electrical resistance. He measured the resistance of his stenographer periodically during the day, and of three other subjects before and after taking the Binet test, and found a rise after the mental work. His interpretation is that mental work causes the resistance to rise. It should be determined whether the rise

was related in his experiment to the relaxation of the subject at the time of measurement as well as to the mental work that preceded.

If the results, however, could be explained entirely in terms of the relative degree of tenseness of the organism, the coefficients of correlation between the extent of learning and the percentage changes in electrical resistance should be negative and high. The crucial coefficients are negative, but are small indeed. Perhaps one of the reasons more material was recalled following hard directions was that the preparatory adjustment of the organism resembled more closely the adjustment in the actual learning situation than the preparatory adjustment involved in the other two conditions. The median changes of resistance from instructions to learning indicate that the resistance rose 1.41 per cent from hard instructions to learning, and fell 2.04 per cent and 2.35 per cent following easy and normal instructions. The rise may be due to a slight relaxation which came when the subject discovered that the task was not as difficult as he had anticipated. The fall after easy and normal directions may on the other hand be due to an increase in tenseness when the subject found that the task was harder than anticipated. The median change from hard instructions to learning was, however, not only an increase in resistance but also a smaller change in contrast to the amount and direction of changes in the other two instances. The increase in resistance after the hard instructions is not sufficient to bring the median or average resistance during the actual presentation of the material up to the median or average resistance following the other types of instructions. The medians are: 86.2 per cent, 91.4 per cent, and 89.2 per cent following hard, easy and normal instructions, respectively. The work of Richter (5) and White (9) indicates that there is a relationship between measurements of apparent electrical resistance and muscular tension. It may be that in this experiment the instructions emphasizing the complexity of the task increased the muscular tension of the organism; those calling attention to the simplicity of the task also increased the tension, but not to the same degree. This interpretation is strengthened by the experimental report of Sears (6). He found that the problems in addition which were made more diffi-

cult, either by increasing the rate of presentation or increasing the complexity of the material, were followed by greater galvanic deflections than were the simpler problems. Perhaps the instructions given our subjects caused them to get set for a relatively difficult or easy learning task.

It is not surprising, therefore, that following instructions emphasizing the complexity of the material there was, on the average, greater recall than following instructions emphasizing the simplicity of the material. Bills (1) found that subjects while gripping a dynamometer learned more than when not gripping it. Stroud (7) found that those subjects who were most tense during the first trials on a maze learned the maze more rapidly than those who were not so tense. We do not mean to suggest that the degree of tenseness of the organism is the sole determinant of the amount of learning; nor do we mean to suggest that the sole determinant of the apparent electrical resistance is the amount of muscular tenseness. We mean only that the amount of learning is to some extent a function of the muscular tenseness and that differences in muscular tension are related to differences in apparent electrical resistance.

Summary

1. Instructions emphasizing the complexity of the learning task are followed, on the average, by apparent electrical resistance lower in proportion to the original resistance than instructions emphasizing the simplicity of the learning task.

2. There is a relation between the complexity of the bodily set and the apparent electrical resistance—the more complex bodily set being accompanied by electrical resistance lower in proportion to the original resistance.

3. The learning is greater following instructions emphasizing the complexity of the learning task than following instructions emphasizing the simplicity of the learning task.

4. Changes in adjustment as indicated by measurements of electrical resistance were less in the shift from instructions to learning when the instructions were hard than when easy or normal.

5. The superior learners tend to have, on the average, higher apparent electrical resistance during learning than the poorer learners.

6. The learners whose electrical resistance increased during rest immediately following recall were superior to those whose resistance decreased during rest following recall.

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LA RELATION ENTRE LES RENSEIGNEMENTS ET LA RÉSISTANCE ÉLECTRIQUE ET L'ÉXÉCUTION

(Résumè)

On a fait des mesures de la résistance électrique sur chacun de huit sujets pendant deux minutes et demie et immédiatement avant que le sujet ait saisi un dynamomètre. Chaque sujet a saisi le dynamomètre dans deux conditions: la première, quand on lui a dit de le serrer dix fois; la seconde, quande on lui a dit de ne le serrer qu'une seule fois. On a demandé à vingt-sept sujets d'apprendre trois codes, chacun avec une série différente de directions; la première appuie sur la difficulté de la tâche; la deuxième sur la facilité de la tâche; la troisième ne dit rien de la difficulté de la tâche. On a fait des mesures de la résistance électrique avant la lecture des renseignements, pendant la lecture des renseignements, pendant l'apprentissage, pendant la période de repos, et pendant le rappel. Les tensions plus complexes du corps, qui se sont montrées dans l'anticipa-

tion de dix serrements du dynamomètre et de la tâche plus difficile de l'apprentissage, ont été accompagnées d'une résistance électrique moins forte en proportion de la première résistance que les tensions plus simples du corps. Les sujets qui ont le mieux appris ont eu en moyenne une plus forte résistance électrique évidente pendant l'apprentissage que ceux qui ont moins appris. Les sujets dont la résistance électrique est devenue plus forte pendant le repos immédiatement après le rappel ont été supérieurs à ceux dont la résistance est devenue moins forte pendant le repos après le rappel.

WHITE

DAS VERHÄLTNIS DER ANWEISUNGEN ZU DEM ELEKTRISCHEN WIDERSTAND UND DER LEISTUNG

(Referat)

Elektrische Widerstandsmessungen wurden bei jeder von acht Versuchspersonen zweieinhalb Minuten gemacht und unmittelbar bevor die Versuchsperson den Dynamometer anfasste. Jede Vp. fasste den Dynamometer unter zwei Umständen an: unter dem ersten musste sie ihn zehnmal drücken; unter dem zweiten musste sie ihn nur einmal drücken. Siebenundzwanzig Vpn. wurden gebeten, drei Geheimschriften zu lernen, deren jede verschiedene Anweisungen hatte; erstens wurde die Schwierigkeit der Aufgabe betont; zweitens wurde die Leichtigkeit der Aufgabe betont; drittens wurde keine Angabe über die Schwierigkeit der Aufgabe gegeben. Elektrische Widerstandsmessungen wurden vor dem Lesen der Anweisungen, während des Lesens der Anweisungen, während des Lernens, während der Erholungszeit, und während des Sicherinnerns gemacht.

Die komplizierteren Körpereinstellungen, die in Erwartung der zehn Drücke des Dynamometers und der schwierigeren Aufgabe vorkamen, wurden von einem elektrischen Widerstand begleitet, der verhältnismässig niedriger im Verhältnis zu dem ursprünglichen Widerstand war als bei den einfacheren Körpereinstellungen. Die überlegenen Lernenden hatten im Durchschnitt einen höhren scheinbaren, elektrischen Widerstand während des Lernens als die schlechteren Lernende. Lernende, deren elektrischer Widerstand während der Erholungszeit gleich nach dem Sicherinnerns zunahm, waren denjenigen überlegen, deren Widerstand während der Erholung nach dem Sicherinnern abnahm.

WHITE

PROFESSOR LEWIN'S CONCEPTION OF LAWS*

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In his paper on "Aristotelian versus Galileian Modes of Thought" (5), as well as in other papers (3, 4) Professor Lewin attempts to state the nature of psychological laws and the exact method of approaching them. In connection with these topics he also pays special attention to statistical methods, which in his opinion are open to serious criticism. Instead of statistical dealing with as great as possible a number of cases, he proposes the single-case approach, which he considers adequate to establish laws of absolute validity. Considering (1) the paramount importance of these topics, (2) the fact that Professor Lewin's ideas have been greeted quite favorably by many psychologists, and (3) that in spite of their brilliant advocacy, their foundation itself is open to crticism, I propose to make in the present paper a critical analysis of them.

PROFESSOR LEWIN'S CONCEPTION OF ARISTOTELIAN VS. GALILEIAN MODES OF THOUGHT

Due to the fact that Lewin's discussion of the nature of scientific laws, of their postulates and method of approach, is closely dependent upon the opposition of the Aristotelian and the Galileian way of thinking, which he discusses at length, I will try first to review this opposition, considering it, however, only from the standpoint of the main topics to be discussed in the present article.

1. *Aristotelian Concepts.* According to Aristotle there are two classes of phenomena: (*a*) lawful and (*b*) unlawful or chance.

> To the young science of physics the Universe it investigated appeared to contain as much that was chaotic as that which was lawful. The lawfulness, the intelligibility of physical processes was still narrowly limited. It was really present only in some processes, as, for example, the course of stars, but by no means in all the fleeting and transitory events of the earth. (5, pp. 144-146)

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The criteria for distinguishing between these two categories of processes were their *regularity* and *frequency* of occurrence. Processes occurring *regularly* and *frequently* are lawful, while the irregular and infrequent ones are simple chance and individual happenings. Between them there is a complete antithesis. Only the first are intelligible and only they form the object of science; the second are simple chance and exceptions.

> In other words, the ambition of science to understand the complex, chaotic, and unintelligible world, its faith in the ultimate decipherability of this world was limited to such events as were *certified* by repetition in the course of history to possess a certain persistency and stability. (5, p. 145)

The method of distinguishing lawfulness from chance and exceptions is by its very nature a statistical one. In order to decide whether a process is lawful or not one has to study it many times, in different places and different moments, and then to compute the *average* of occurrence in all these observations and to ascertain its regularity and frequency. With only one observation one is absolutely unable to make such a distinction between law and chance for the simple reason that the occurrence of the process in question can be limited to this case only and cease to occur in other cases. In other words it can be simple chance or exception, which by its very nature is the opposition and contradiction of lawfulness. Once the average is computed, this average is considered not as an *abstraction,* as it is in fact, but as revealing the real nature and intimate essence of processes and things. The nature of individual things and processes is thus identified with the nature of the class of things. Class and things become identities, the class being the essence of things, while things are the contingent appearance of class. The fact that things and processes differ largely between themselves is attributed to errors of observation of their appearance and not at all to their natural variation. The "essential nature of thing" is ". . . exactly the abstractly defined class (that is, the sum total of the common characteristics of a whole group of objects)."

2. *Galileian Concepts.* The Galileian physics, the conception of laws, when compared to that of Aristotle, is more pretentious. It

claims that everything in the world is lawful. Chance is absolutely excluded. The Universe is only one and not two. It contains only lawful processes and not lawless and chance.

> The outlook of a Bruno, a Kepler, or a Galileo is determined by the idea of a comprehensive, all embracing unity of the physical world. The same laws govern the courses of stars, the falling of stones, and the flight of birds. (5, p 148)

The Universe is perfectly the same everywhere and all the time. There are no such classes as lawful and chaotic processes. There are only lawful processes, with universal application and validity, because all things in the Universe are identical in their very nature. The principles of *homogeneity* and *continuity* replace those of discontinuity and heterogeneity, common to Aristotelian thinking.

The criteria of law are not mere regularity and frequency. Law is "the principle of exceptionless validity" (5, p. 166). The law characterizes something which happens always, without exception. Exceptions are not allowed. The laws hold absolutely.

The Aristotelian antithesis between lawfulness and individual cases disappears. The individual case is no more an exception. It is only one case of the applicability of the Universal law. Class is simple addition of individual cases and not essence or nature of such things. Every individual case represents entirely in itself its nature and essence. Consequently the full knowledge of a single case is absolutely satisfactory for ascertaining certain relations, that is to say, certain laws. To oppose class and particular case is nonsense. What holds for an individual case holds for class and for everything else. There is no place for exceptions.

> *It depends upon keeping in mind that general validity of the law and concreteness of the individual case are not antithesis, and that reference to the totality of the concrete whole situation must take the place of reference to the largest possible historical collection of frequent repetitions.* (5, p. 175)

In order to draw absolutely valid laws from the study of a single case, we need not, however, limit our knowledge to the mere appearance of things, like Aristotle. We need to go beyond their immediate perception and to study the deeper functional relations. We must make a "conditional-genetic" study and not a simple de-

scriptive one. In considering the distinction between *phenotype* and *genotype,* used in biology, Professor Lewin thinks that Galileian physics studies the processes genotypically, while Aristotle used to study them phenotypically.

3. *Applications to Psychology.*

> It is the increased desire, and also the increased ability, to comprehend *concrete particular cases,* and to comprehend them fully, together with the idea of the *homogeneity* of the physical world and that of continuity of the properties of its objects, which constituted the main impulse to the increasing quantification of physics (5, p. 150).

The same has to be done in psychology, if it is to become an exact science, like present physics. It is not now such an exact science and the only reason is that the Aristotelian mode of thought has dominated and still dominates too much psychological thinking.

> The concepts of psychology, at least in certain decisive respects, are thoroughly Aristotelian in their actual content, even though in many respects their form of presentation has been somewhat "civilized," so to speak. (5, p. 165)

a. Aristotelian mode of thought in psychology. Psychology, like Aristotelian physics, still distinguishes between lawful phenomena and individual cases. Its object as exact sicence is, of course, only the first field of lawfulness. What is not regular and frequent is considered without any importance from its scientific point of view.

> If, for example, one shows a film of a concrete incident in the behaviour of a certain child, the first question of the psychologist is "Do all children do that, or is it at least common?", and if one must answer this question in the negative the behaviour involved loses for that psychologist all or almost all claim to scientific interest. To pay attention to such an "exceptional case" seems to him a scientifically unimportant bit of folly. (5, p. 155)

The individual event seems to him fortuitous, unimportant, scientifically indifferent (5, p. 151). Such facts do not belong in the field of lawful events and psychology, so far as it claims to be exact science, is concerned only with this field.

> As a matter of fact, any psychology that does not recognize lawfulness as inherent in the nature of the psychic, and hence in *all* psychical processes, even those occurring only once, *must* have criteria to decide, like Aristotelian physics, whether or not it has in any given case to deal with lawful phenomena. (5, p. 152)

These criteria are the same as in Aristotelian physics: regularity and frequency. Of course the best way to use these two criteria is the statistical.

> The statistical procedure, at least in its commonest application in psychology, is the most striking expression of this Aristotelian mode of thinking. In order to exhibit the common features of a given group of facts, the *average* is calculated. This average acquires a representative value, and it is used to characterize (as "mental age") the properties of 'the' two-year-old child (5, p. 153)

The individual and particular cases are wholly disregarded, such facts being considered the antithesis of lawfulness and science. Because

> . . . lawfulness and individuality are considered antitheses . . . it appears hopeless to try to understand the real, unique, course of an emotion or the actual structure of a particular, individual personality. It thus reduces one to a treatment of these problems in terms of mere averages, as exemplified by tests and questionnaires. (5, p. 155)

Because averages are easier found in the field of sensations and reflexes, psychology became chiefly a study of sensations and reflexes. The more superior and complicated processes, like emotions, will, temperament, personality, have been avoided because they failed to be submitted to a treatment in averages. Often they have been considered unlawful and unscientific, because of failure to use a statistical approach. Since psychological laws are based on principles of regularity and frequency and not upon the principle of exceptionless validity, the psychologists did not and do not claim to find "absolute laws," holding without exception. They are fully satisfied with mere "regularities." "The demands of psychology upon the *stringency* of its propositions go no farther than to require a validity 'in general' or 'on the average,' or 'as a rule' "

(5, p. 156). Let the subject have, for instance, a headache and the law no longer applies.

It is still more absurd to apply such laws to individual and concrete cases. They can be of no value for such concrete cases, which, by their very nature, being mere "accidents," are just the opposite of lawfulness. Thus the laws hold only in the abstract, for the average, in their ideal spheres.

b. Galileian mode of thought in psychology. What we need to do at present in psychology is just what has been done by Galileo, Kepler and Newton in physics, that is to introduce the principle of absolute homogeneity and continuity, together with the principle of universal lawfulness and exceptionless validity. Lewin thinks that we already are on the way. Freud, for instance, has contributed largely to

> the abolition to boundary between normal and pathological, the ordinary and the unusual and hereby furthered the "homogeneization" of all the field of psychology. (5, p. 159)
>
> . . . reflexology and behaviorism have contributed to the homogeneization of "man and animal" and of "body and mind." (5, p. 159)

Since the psychological processes are absolutely homogeneous and continuous, the same laws, of course, hold for everything and at all times.

> It is coming to be realized that every psychological law must hold without exception. (5, p. 159)
>
> The theory of general validity permits of no exception in the entire realm of the psychic, whether of child or adult, whether in normal or pathological psychology. (5, p. 161)

In order to have such laws we no longer need to study "regularities" in a statistical way. All individuals, in all situations, being homogeneous, we need only to study *one* individual in *one particular case* and to apply then the conclusions to all other cases, be they children or adults, normal or insane, etc.

> Even a "particular case" is then *assumed,* without more ado, to be lawful. Historical rarity is not disproof, historical regularity not proof of lawfulness. For the concept of lawfulness has been quite detached from that of regularity; the

> concept of the complete absence of exceptions to laws is strictly separated from that of historical constancy. (5, p. 162)

Of course, in order to obtain such laws we must go beyond the study of simple appearance of psychological phenomena in our immediate perception of them and study them genotypically and not phenotypically.

Discussion

I must confess that I do sympathize deeply with Professor Lewin's program. After all, what psychologist will not be delighted to hear that his discipline is able to have laws of exceptionless and universal validity and that his propositions are designed to hold forever! The only cause for regret is that as yet Lewin has demonstrated only the "probabilities" of such laws, drawn from single-case studies, and that practically all the laws formulated in his ingenious and stimulating researches are simple irregularities, like all the rest, and drawn by using a rather high number of subjects, like all other psychologists. I would have much preferred at least one "absolute law," with exceptionless validity, instead of the demonstration that all the laws can and ought to be absolute, holding forever. The reason for my preference is simple. Lewin is neither the first nor almost certainly the last to prove the possibility of exceptionless laws. What else have the associationist psychologists, like Stuart Mill, Spencer, E. Mach, Ebbinghaus, etc., claimed to prove than just the possibility of laws holding forever! Some have already claimed to have found and proved such laws. However, what is the main result of Lewin's researches—I say researches and not theory—if not just the proof that associationistic psychology is entirely inadequate and its laws incorrect! If so, and I think that it is so indeed, who can prove that Gestalt psychology and Lewin himself have the last word? But it is not this general criticism which determined my skepticism toward his program.

1. *Principle of Homogeneity.* The main idea or postulate in Lewin's discussion is, to be sure, the principle of homogeneity. The validity of all the remaining principles, such as absolute laws, exceptionless validity, drawing valid conclusions by studying a single case, etc., depend upon it. If the principle of homogeneity

and continuity is true all the affirmations and deductions concerning the other principles hold; if not, they simply lack foundation.

Does this principle of universal homogeneity and continuity hold? Until we know the world thoroughly a scientific answer is not possible. It might be yes, it might be no. In other words the principle of absolute homogeneity is not so much a matter of science as more a matter of philosophy. The reason for this affirmation is that after all it is possible to have a science, certainly one with relative laws and truths, assuming only a relative homogeneity and continuity. That is to say that science can have as postulates a certain degree and combination of homogeneity and heterogeneity, continuity and discontinuity, determinism and indeterminism. In fact, as we shall see, this is the modern trend in all sciences, including physics itself.

a. The main argument, or rather the only argument, upon which Lewin bases the universality of homogeneity is its clear recognizance by Galileo, Kepler, Giordano, Bruno, etc. Assuming that the principle was actually demonstrated by these scientists and philosophers (a fact quite doubted by modern physics), does it follow that this same principle holds also for biological, psychological, and social sciences? It does, if the phenomena studied by these sciences are in their very nature identical with those studied by physics. Is this the case? Who can answer definitely? Some thinkers answer in the affirmative and many others in the negative. Lewin himself recognizes the delicacy and difficulty of the problem and, though inclining more toward the first answer, prefers to leave the question open. That means, however, that all the rest of his arguments remain open, too.

b. But the principle of absolute homogeneity, as I said, is largely doubted in physics itself, a fact realized by Lewin himself. He thinks that this does not affect his arguments, which I doubt very much. Doubt of absolute homogeneity and thorough continuity is raised by some of the most outstanding physicists, such as Planck (6) and other exponents of quanta theory, L. de Broglie (2), Eddington (1), Heisenberg [see Eddington (1)] etc. H. Poincare (8) long ago doubted it. The universe of modern physics is an unknown combination of unknown degrees of homogeneity and heterogeneity, continuity and discontinuity, determinism and indeterminism. More adequately stated, the universe is a certain X,

beyond such antitheses as continuity-discontinuity, homogeneity-heterogeneity, etc. These may be given only in our imperfect mind which, like Tantalus, tries to cover the infinite, deprived even of adequate categories of thinking. That means that "absolute laws" and "exceptionless validity" might be after all a mere product of our mind and nothing else. In fact all the known laws are exceptionless in a certain relative degree only. In other words the physical laws themselves are nothing else than "statistical regularities" (Heisenberg) and never absolute and exceptionless conformities. If one is to draw conclusions from physics, why not draw then them from modern physics? Why go back to Galileo and Kepler? Modern physics and physicists by no means deserve less credit than those of the Renaissance.

c. The main idea in biology and psychology, as well as in the social sciences, seems to be less the idea of homogeneity than that of *variation* and individuality, considered—at least at present—both qualitatively and quantitatively. There are no two individuals perfectly equal, that is to say homogeneous. All are different. One and the same biological or psychological law can not be applied in one and the same manner to all individuals. In order to be adapted to individual *A* or *B,* it must always be corrected. The law by its very nature is always and *necessarily* an "abstraction," holding "in general" and "on the average," and *necessarily* needs correction and attenuation in dealing with concrete individuals. Moreover, it can be applied successfully, even when so corrected, providing that a certain identity of conditions is satisfied. If not and the subject has, for example, "headache," as Lewin says, then the law no longer holds. (This does not mean that chaos reigns. It means only that the phenomena are different and consequently different laws are to be applied.)

In order to find the personal coefficient, which we must have in order to apply a law to individual *A, B,* or *C,* we need to know the general rules of variation and the place of these individuals in the variation of the phenomenon to which the law refers. It is fortunate from the standpoint of our science that laws of variation exist and the place of individuals in the variation can be ascertained. The variation curves and their mathematical expression and interpretation, first used in psychology by Galton and developed especially

by Pearson, Cattell, Thorndike, etc., are important instruments in this work.

Taking into consideration this fact it seems to me—however paradoxically it might appear from the standpoint of Lewin's logic—that statistical method was designed, at least at its origin, to deal not so much with abstractions as with individuals and individual differences. In fact Galton's work was mainly a protest and reaction against the psychology of the time, dealing with sensations, perceptions and associations in general and in the abstract. He studied them as they took place in concrete individuals. Wundt's greatest handicap was not that he obtained regularity by repetition of the experiment on one and the same individual, as Lewin claims. The great shortcoming of his psychology was that he did not obtain regularity and variation for a great number of individuals as well.

The story of Cattell's work on reaction time in Wundt's laboratory is well known. According to Wundt's principles and methodology he had to study reaction time in general and not as it occurs concretely in different individuals. The fact that he had to study more than one individual (in general between three and five), as was the custom, did not mean that he had to pay any attention to individual differences as such. His object was only to obtain more reliable data, free from errors of observation. The individual differences were classified more or less in the same category with the errors of observation and the only aim of repetition was to obtain more constant and valid results, which were believed to be given by the arithmetical mean of all experiments. Nobody realized that individual differences are not so much errors of observation as significant results of the natural variation. Cattell was the first to realize this. His idea and method were finally allowed by Wundt, although he did not have great confidence in such a *"ganz amerikanisch"* way of thinking. Cattell consequently gave credit to Galton and considered the individual differences valuable for study in themselves. He renounced, too, the idea of absolute homogeneity—to use Lewin's expression—so firmly believed by Wundt and still characteristic of German psychologists, including Lewin himself. Instead Cattell took the idea of variation, accepted and followed by so many American psychologists.

In order to give a more concrete illustration, may I recall the

quotation from page 400 in connection with the film showing a concrete incident in the behavior of a certain child. Lewin claims that the first question asked by psychologists usually is:

> Do all children do that, or is it at least common? And if one must answer this question in the negative the behavior involved loses for that psychologist all or almost all claim to scientific interest. To pay attenton to such an "exceptional case" seems to him a scientifically unimportant bit of folly.

This may be provided that the psychologist is a poor statistician. If not I dare say that he would say: "Well, that is very fine, but you certainly plan to complete your study to see how other children behave in this situation in order to know the whole variation." In other words he feels that one should study variation, if possible with curves, coefficients, etc. These requirements imply the obvious impossibility of drawing valid conclusions, holding for all individuals, by studying only one individual. As long as the individuals are different in their nature absolutely valid conclusions based on one individual are both impossible and without sense, or "at least a bit of folly," to use Lewin's own expression. After all, the fact that individual *A* does an arithmetical operation in one hour while individual *B*, of the same age and mathematical training, completes the problem in a couple of minutes means a rather fundamental and radical difference, however "phenotypical" it might be.

d. I remember one discussion with an American psychologist who visited the German psychological laboratories. He was rather impressed by many researches in progress. One fact, however, surprised him very much, namely that none of the studies on animals, for instance, were ever based on more than two, three, or four subjects. He was accustomed to use at least 20 or 50 subjects in order to determine approximately the natural variation. Having noticed the same fact in his previous reading of German literature, he took this opportunity to ask why so few animals were used. His surprise increased when the German psychologists answered by asking: "But why more than three or four animals? Aren't they enough?" In this respect I consider Lewin less "German." In his researches he never used only one subject, as he claims in his theory. Moreover, he has sometimes used over 10 or 20 subjects, a fact which means a great

and significant progress. Therefore, he certainly is more "American" than his former colleagues. However, I think that he is still inclined, in his papers, to identify statistics with computation of averages. I recall the quotation from page 401.

> The statistical procedure, at least in its commonest application in psychology, is the most striking expression of this Aristotelian mode of thinking. In order to exhibit the common features of a given group of facts, the average is calculated. This average acquires a representative value, and it is used to characterize (as "mental age") the properties of "the" two-year-old child. (5, p. 153)

This view might characterize some few psychologists who understand little statistics. It is, however, mistaken. Statistics is not so much the study of averages as the study of *variation*. Simple averages are used adequately only for obtaining reliable data from many observations, made on one and the same individual. Even then the probable error and standard deviation are required. They are not used adequately for the study of many individuals. Variation curves and variation coefficients, mode, quartiles, etc. are or should be used. Finally, if some statisticians are poorly trained and use statistical method badly, we can not throw out the baby with the bath. Therefore if somebody claims that "The average acquires a representative value and is used to characterize (as 'mental age') the properties of 'the' two-year-old child," he is giving an entirely wrong interpretation of the statistics. The fact that statistics are misinterpreted does not mean, of course, that statistics in itself is wrong. If another goes farther and identifies the "average" not only with the nature of the phenomena, but with their "cause" also, using the average as an explanatory principle, which Lewin claims is frequently done, he certainly is not only a bad statistician, but a bad scientist as well.

e. Is it possible, according to the above discussion, to determine laws with "absolute validity" from the study of a single case? Obviously not, because this means that all the other cases are identical and homogeneous. This means that there are no real qualitative differences between individuals and there is no variation, regardless of whether it is Gaussian or not. If, on the contrary, such qualitative differences and such variation exist (and all our knowledge proves that they exist indeed), to hold the possibility of reaching

"absolutely valid" conclusions, by studying only one case seems to be without sense. It is obviously contrary to facts. It is true, however, that Lewin speaks about variation and individual differences, both qualitatively and quantitatively. How this fact agrees with the idea of absolute homogeneity and absolute laws, which hold forever, he does not tell. As far as I am concerned personally I must confess that I am unable to understand.

2. *The Distinction between Phenotype and Genotype.* It is true that according to Lewin in order to draw "absolute laws," "for the child as well as for the adult, normal or abnormal," by simply studying completely one case, we must go beyond "simple appearance" of the things, beyond their "immediate perception" and study them deeply, in their very nature and in their constant and permanent relations. In other words we need to study the things and the processes in their "conditional-genetical" dynamism and not in their "historico-geographical" relations, that is to say "genotypically" and not "phenotypically."

Let assume for the moment that the idea of perfect homogeneity and continuity is true and that the possibility of drawing conclusions from the study of a single case holds. Providing that such things *ideally* are possible, are they also *practically* possible? That is to say, can we have "full knowledge" of the case? It is true that some contemporary philosophers claim such a knowledge. I have in mind especially Bergson and Husserl, the most brilliant thinkers of our generation. However, Einstein proved mathematically that even the most elementary, constant, and universal laws are relative to certain parts of the universe and their validity holds only for that part and not at all for the others. The speed of falling of a stone is different on different planets. If someone prefers to go with Husserl, somebody else prefers to go with Einstein. Nobody can claim that only he is right and all the rest wrong. The matter is one of philosophy of knowledge, depending largely upon a metaphysical foundation, and the view one chooses is a question of training as well as of taste and personal preference.

Probably Professor Lewin did not intend to be engaged in such a discussion, which is very valuable but with no hope of reaching a decision. In fact he uses the terms "absolute laws" and "full knowledge" in German papers only and not in the American one.

He also does not claim at all that some of his laws are absolute or some of his cases "fully" known. I am inclined to think that what he really means by genotypical knowledge is the most exact and valid knowledge which is possible and which obviously goes beyond the immediate perception of things, takes a deeper insight in the more permanent and constant relations and structure of things, is more controlled, often helped in its precision by apparatus, etc. But is not this knowledge just the ideal of every scientist and psychologist? Lewin says, for instance, that associationistic psychology was based mostly upon phenotypic knowledge, while Gestalt psychology is based mostly upon genotypic knowledge. I quite doubt this. I certainly do think that present psychology, when compared to the psychology of Herbart, and even Wundt, Ebbinghaus, Ribot, etc., is more advanced in its method and is able to collect more valid and reliable data. I doubt, however, that behaviorism, for instance, is mostly phenotypical while Gestalt psychology is mostly genotypical, as Lewin claims.

The fact that statistics is mostly phenotypical is doubtful, too. Probably some statisticians are not careful enough in collecting and accumulating their data. Does this mean that statistical method can be based upon phenotypic knowledge and not at all upon genotypic knowledge? Quite the contrary seems to be true. Lewin gives one example in order to show the phenotypic foundation of the variation curve and the inherent mistakes of such a foundation (3). Let us suppose, he says, a series of four kinds of beans. The length of the first kind varies from 10 to 19 units, the second from 17 to 28 units. etc. Mixed together they have a Gaussian variation between 10 and 40 units. Statistically, therefore, one concludes that they are all one kind of beans and not four kinds. Assuming that such cases are possible, what statistician uses different variables in constructing a variation curve? Is not the isolation of the variable the postulate of any curve?

3. *Regularity Versus Exceptionless Validity.* According to Aristotle the criteria of lawfulness are regularity and frequency; according to Galileo the criterion is the principle of exceptionless validity. Between these two categories of criteria there is a definite contrast. The first criteria, regularity and frequency, hold only in some cases, for facts and processes which are very common to the majority of

individuals; the principle of exceptionless validity holds always, regardless of the fact that the processes are common and frequent or specific and seldom. This contrast between regularity and frequency on one side and exceptionless validity on the other side holds only in regard to Aristotle and Galileo, and not at all in respect to present status of knowledge. It seems to me that Lewin fails to make a distinction between two quite different things: *frequency* of occurrence of phenomena and *conformity* of occurrence. The notion of law is based upon conformity of occurrence and has nothing to do with frequency of occurrence. Visual or acoustic perceptions are characteristic of more than 99% of individuals, while Daltonism characterizes only a small number. The Oedipus complex, for instance, probably has a position somewhat between these two extremes. The same is true of many other psychological phenomena. However, the fact that visual phenomena occur in almost 100%, the Oedipus complex probably in less than one-third of individuals, and Daltonism in very few does not mean that visual perceptions are lawful, while Daltonism is an exception. Daltonism is lawful, too. It is lawful because if certain conditions are given, namely a certain constitution and a certain environment, Daltonism appears in all the cases. The fact that certain conditions, that is to say certain heredity and certain environment, appear in 100%, 80%, 40%, 10%, or 1% and less than 1% is a quite different thing. From the standpoint of lawfulness it is quite without importance.

In fact Professor Lewin not only does not deny this fact, but on the contrary points out very clearly the above distinction between frequency of occurrence and conformity or exceptionless validity. I do not believe that modern psychologists are unaware of this distinction, as he claims, or that in order to have laws they require frequency of occurrence and not conformity of occurrence, or exceptionless validity, to use his term. I think also that Lewin's opposition of regularity and exceptionless validity is somewhat inadequate. In fact most people mean by regularity conformity of occurrence. Regularity being so understood exceptionless validity is nothing else than just the negative expression of regularity. Absolute regularity means exceptionless validity and vice versa. They both express one and the same thing, seen from opposite directions. Lewin himself uses both expressions.

4. *Discrepancy between Theory and Researches.* Any one who has read Lewin's experimental researches realizes the discrepancy between his theory of science (I would say has philosophy of science) and his method. After all he never claims "absolute validity" for his laws. Also, instead of using one case he uses many. In fact he claims to have demonstrated merely the *possibility* of absolute laws, formulated by fully studying one case. The translation of *possibilities* into *facts* is still a problem of the future.

The object of the present paper was to question not only this future translation, but also to question its theoretical possibility. This being the aim of the paper, it would be a misunderstanding to conclude that I question Lewin's experimental method and researches. Quite the contrary is true. The writer is a great admirer of these methods, researches, and results, and classifies them among the most fundamental contributions to contemporary psychology. Moreover, he might confess that he wrote the present critique because it occurred to him that some people disregard Lewin's method and results because of their skepticism toward his theory.

I do also heartily agree with Lewin that many present psychologists are too much preoccupied with extensive rather than intensive studies, paying attention more to validity through repetition and less to validity through careful control of the individual cases. It certainly is necessary to strike a better balance. In order to prove this necessity I do not think, however, that we must go back to Galileo and Aristotle. We can better realize the present needs by comparing the achievements by one method and the other. Nor do I think that we must go from one extreme to another, from extensive statistical studies to intensive clinical and individual studies, and from too much statistics to complete abolition of statistics. We need to keep both methods. Both are rather imperfect and the only way of getting better results is to use them both, as far as possible simultaneously and in collaboration, controlling the results obtained by one method with the results obtained by the other. Even in such a case the results will remain far from the ideal of full knowledge. It is certainly too bad that the results are so meager! But we must not forget that what we have today is better than others had yesterday. And after all, as Poincare (7) said, it is all we have.

Summary

The paper attempts a critical analysis of Professor Lewin's conception of scientific laws. According to Lewin the basic principles of scientific law are exceptionless validity and absolute homogeneity. A phenomenon is lawful when it occurs without exception. This exceptionless validity holds owing to the fact that the phenomena are perfectly homogeneous and continuous both in time and space. What holds for one phenomenon holds for all the rest. Consequently in order to formulate one law the full study of a single case is entirely satisfactory. The findings can be generalized to the others. Statistical study dealing with more than one case is not at all necessary.

Against the principle of absolute homogeneity, exceptionless validity, and sufficiency of the knowledge of a single case the author raises the principles of relative homogeneity, probability, and necessity of statistical dealing with a satisfactory number of cases. The phenomena are never absolutely homogeneous. They are always found in some combination of homogeneity and heterogeneity, continuity and discontinuity. The fact is clearly recognized not only in social, psychological, and biological sciences, but also in physical sciences. Since the phenomena are never absolutely homogeneous and continuous, it is impossible to have laws holding always and forever. Only relative laws, representing statistical probabilities, are possible. The formulation of such laws always requires a statistical study of a large number of cases.

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LA CONCEPTION DES LOIS SCIENTIFIQUES DU PROFESSEUR LEWIN

(Résumé)

On fait une analyse critique de la conception des lois scientifiques du Professeur Lewin. Selon Lewin les principes fondamentaux de la loi scientifique sont la validité sans exception et l'homogénéité absolue. Un phenomène est consideré comme accessible à une loi à la condition qu'il se produit sans exception. Cette validité sans exception est possible en vertu du fait que tous les phenomènes sont parfaitement homogènes et continus en temps et en espace. Ce qui est valide pour un phenomène est valide pour tous les autres. En conséquence la connaissance complète d'un seul cas suffit pour la formulation d'une loi. Les résultats peuvent être generalisés pour le reste. L'étude statistique qui traite plus d'un cas n'est pas du tout nécessaire.

En opposition avec les principes de validité sans exception, d'homogénéité absolue et de la suffisance de la connaissance d'un seul cas, on soulève les principes d'homogénéité relative, de probabilité et de la necessité de traiter un grand nombre des cas d'une façon statistique. Les phenomènes ne sont jamais absolument homogènes. Ils existent toujours dans une certain combinaison d'homogénéité et d'héterogénéité, de continuité et de discontinuité. Le fait est clairement reconnu non seulement dans les sciences sociales, psychologiques et biologiques, mais même dans les sciences physiques. Les phenomènes n'étant jamais absolument homogènes et continus, il est tout a fait impossible d'avoir des lois absolues, valides pour toujours. Il n'est pas possible que des lois relatives, representant des probabilités statistiques. La formulation de telles lois relatives exigent l'étude statistique des plusieurs cas.

MARGINEANU

PROFESSOR LEWIN'S BEGRIFF DES WISSENSCHAFTLICHEN GESETZTES

(Referat)

Professor Lewin's Begriff des wissenschaftlichen Gesetzes wurde in diesem Aufsatz einer kritischen Analyse unterzogen. Die fundamentalen Prinzipien des wissenschaftlichen Gesetzes sind nach Lewin ausnahmslose Geltung und absolute Homogeneität. Eine Erscheinung ist gesetzmässig, wenn sie ohne Ausnahme erfolgt. Diese ausnahmslose Geltung ist durch die vollkommene Homogeneität und durch die zeitliche und räumliche Kontinuität der Erscheinungen bedingt. Was für ein Phänomen gilt, das gilt für alle. Demzufolge ist eine erschöpfende Untersuchung eines einzigen Falles für die Formulierung des Gesetzes gänzlich hinreichend. Die Befunde können verallgemeinert und auf andere Fälle übertragen werden. Eine statistische Untersuchung, die mit einer Anzahl von Fällen arbeitet, ist nicht notwendig.

Den Prinzipien der absoluten Homogeneität, der ausnahmslosen Geltung

und dem Prinzip der Genügsamkeit der Erkenntnis eines einzigen Falles stellt der Verfasser die Prinzipien der relativen Homogeneität, der Wahrscheinlichkeit und der Notwendigkeit einer statistischen Behandlung einer ausreichenden Anzahl von Fällen gegenüber. Die Erscheinungen sind niemals absolut homogen. Sie werden immer in einer Kombination von Homogeneität und Heterogeneität, Kontinuität und Diskontinuität angetroffen. Diese Tatsache wurde nicht nur in den Sozialwissenschaften, in den psychologischen und biologischen Wissenschaften klar erkannt, sondern selbst in den Naturwissenschaften. Da die Erscheinungen niemals absolut homogen und kontinuierlich sind, ist es ummöglich, Gesetze aufzustellen, die für alle und für Ewigkeit gelten. Nur relativ Gesetze, die statistische Wahrscheinlichkeiten darstellen, sind möglich. Die Formulierung eines solchen Gesetzes erfordert immer eine statistische Untersuchung einer grossen Anzahl von Fällen.

MARGINEANU

THE FORMATION OF VISUAL CONDITIONED REFLEXES AND THEIR DIFFERENTIATION IN INFANTS*

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I. Introduction

There is no doubt that the functions of the nervous system are developed in a strict genetic order. Among the different nervous processes taking place in the central nervous system, the most important ones in infants are the processes of the higher levels of nervous activity. Being completely absent in the earliest stage, conditioned-reflex activity unfolds and gradually becomes more complex. The degree of its development is always closely connected with the age of the child.

In the course of our experiments we have established that during the first two or three weeks of its life the child is unable to form connections between the various phenomena of the surrounding world; his central nervous system (cerebral hemispheres) is not able to form conditioned reflexes.[1] In their studies of the genetic development of the child Figurin and Denisova (2) stated that the first natural conditioned reflex is formed by the end of the first month, when the child is placed in a feeding position.

In that case the irritation of the labyrinth serves as a conditioned excitor that initiates a digestive conditioned reflex expressed in sucking movements. Thus the labyrinth is a receptor which initiates the first conditioned reflex in man.

This fact makes doubtful the suggestion expressed by Krasnogorski

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[1]It has been shown that under certain circumstances conditioned responses can be established in infants within a few days after birth. See Marquis, D. P., Can conditioned responses be established in the newborn infant? *J. Genet. Psychol.*, 1931, **39**, 479-492. The article was evidently not available to the authors of the present paper. D. P. M.

(5) and accepted by Peiper (7) and Helmreich (3) that *"bei normalen Säuglingen und in dem ersten zwei, drei Lebensmonaten die kortikalen Innervatzionen so wenig entwickelt, dass die bedingten Zusammenhänge überhaupt nicht gebildet werden können."*

In the course of our studies on artificially produced conditioned auditory reflexes carried out under strict experimental laboratory conditions, we found that the conditioned auditory reflexes in infants were formed during the first half of the second month. The results obtained are given in our previous work "On the Development of Early Combined Reflexes and Differentiations of Auditory Stimuli in Infants" (4).

Besides these conditioned auditory reflexes a child forms in a comparatively short time a great number of different conditioned responses by means of other receptive organs. Among the latter the visual conditioned reflexes are of the greatest interest. Brief results of our studies on the visual conditioned reflexes in infants are given in the present paper.[2]

Methods and Material

The experiments were performed daily during feeding time at the Reflexological Laboratory of our clinics. From forty to a hundred experiments were carried out with every child. The children were brought to the laboratory in turn and placed in the experimental crib isolated from outer influences. The crib was equipped with different optical, auditory, and tactual signals that could be put into action at the moment required.

In the present investigation colored electric bulbs were used as visual stimuli. They were placed behind the child and gave a uniform light inside the cabinet.

As a non-conditioned stimulus we used a bottle of milk which had been filtered, the contents of the bottle corresponding with the amount usually consumed at that age.

The experiment consisted of the following: A few seconds before the milk bottle was given a light of a definite color was switched on. The light continued during the time when the child was feeding, i.e., from ten to forty seconds. The light and the bottle were simultaneously withdrawn. After an interval of fifteen to thirty

[2]Dr. Solovyeva also took part in the experiment.

seconds the light was again put on and the milk bottle returned. In each experiment these combinations were repeated from one to six times. The intervals between the presentation of the light and the giving of milk were seven seconds in the first experiment and from five to eight in the later ones. At first, when the conditioned reflex had not yet been formed, the child began to produce sucking movements only at the moment when his lips came into contact with the bottle. But when the conditioned reflex had been formed, the movements appeared as soon as the light flashed, before the milk bottle (non-conditioned stimulus) was presented. Thus in our experiments the reaction of the mouth expressed in sucking movements served as an indicator of conditioned reflexes.

The recording of the salivary reaction during the first three months of a child's life is extremely difficult due to the insufficient flow of saliva and to the extreme sensitiveness of the mucous membrane of the mouth, which did not allow the use of a salivary capsule.

For the recording of the sucking movements on the kymograph a vacuum rubber ball was used which was fixed under the child's chin and connected with a rubber tube to a Marey drum. The tracing of curves was used during only part of the experiments, as the fixing of the ball on the child's head produced unnecessary excitement. In each experiment careful records were kept of the child's behavior in response to different signals. Throughout the course of the experiments the children remained quite healthy and corresponded to the typical norms of the age.

As material for our studies we used six children from 14 days to 1 month 17 days old, from the normal department of the Clinic of Pedology and Children's Neuropathology.

The age of the children is shown in Table 1.

TABLE 1

No.	Sex	Name	Date of birth	Date of experiment	Age at beginning of experiment Mos.	Days	Conditioned light stimulus
1	M	B.C.	18/3/29	2/4/29		19	yellow
2	M	S.F.	31/3/29	17/4/29		17	yellow
3	F	B.H.	14/10/29	1/11/29		17	yellow
4	F	G.P.	19/1/29	21/11/29	1	2	yellow
5	F	E.F.	22/1/30	17/3/30	1	15	yellow
6	F	H.T.	1/1/30	17/2/30	1	17	red

III. The Experiments and Results

During the first experiment some of the children showed a characteristic reaction of orientation, in turning the head towards the source of light and sometimes even refusing the milk bottle. At the beginning when lights appeared all of the children closed their eyes, but after a time this reaction was not observed. Taking into consideration the fact that with closed eyes, as while crying, for instance, the light cannot act on the eye, we were obliged to wait for a quiet moment and then noiselessly switch on the lamps. Therefore the intervals between different combinations varied from fifteen seconds to 1.5 minutes.

Further experiments showed that even with the eyes shut the child feels the sudden lighting and therefore can react to it.

The general condition of the child played a most important part during the first stages of the conditioned reflex. A slight elementary excitement proved to be best for the formation of conditioned reflexes. On the contrary, complete rest and immobility, as well as crying or chaotic movement of the body, added much difficulty to the analysis of the reaction.

In our first experiments the conditioned stimulus—light—sometimes produced a definite orientative reaction, as we have already mentioned. The sucking movements appeared only upon the contact of the lips with the milk bottle. Such an indifferent response to the light (before the food support) continued up to the second half of the second month of the child's life. After that period we could note the formation of some new reactions as a response to the conditioned stimulus. The most interesting among them was the appearance of an inhibition of movement at the moment of lighting, that lasted until the lips touched the milk bottle. The gradual development of this inhibition is expressed in an almost complete cessation of whimpering and crying, in immobility of the body and limbs, and in a modulation in voice sound provided the latter occurred in a state of calm. Beside that, a slight twitching of lips and movement of the head toward the light were noted from time to time. This stage in the formation of the conditioned reflexes was evidently present in five out of the six children under experiment and represented the inhibition period. The absence of this period in one

of the children (No. 4) can be explained by its calm state during experimentation.

The development of inhibition is very gradual. At first we could observe inhibition once or twice in the course of experimentation, to a very limited degree. Then inhibition became more pronounced in quality as well as in quantity and the child in response to every stimulus answered with a very expressive cessation of all movements.

Soon after the inhibition period comes the last stage—when the conditioned reflex is being formed. During the first stage the light stimulus either produced an orientative reaction or left the child indifferent. During the second it caused inhibition and in the latest, the third stage, it played the rôle of the conditioned stimulus. The conditioned reflex itself is expressed by intensive sucking movements, which appeared on presentation of the light and continued until the reception of the unconditioned food stimulus.

In some experiments, especially in the case of children with a pronounced food reaction, spontaneous sucking movements may take place in intervals between two different signals. In such cases the presence of the conditioned reflex can be detected by the intensification of the sucking movements. There is, however, the possibility of waiting until the spontaneous sucking movements disappear at the moment when the child begins to whimper slightly. A conditioned stimulus given at that moment always produces a clear and unmistakable conditioned reflex at the stage when the latter has been formed.

Our results are recorded in Table 2.

Inhibition of general movements as well as the conditioning of reflexes develops gradually. We notice conditioned reflex once or twice in each experiment as slight lip or sucking movements. The latter became more extensive with every new experiment, appearing more often with the conditioned stimulus. After the formation of the conditioned reflex the child responds to all the conditioned stimuli by sucking movements. We may consider our first experiment in which the child reacted by conditioned sucking movements to all our conditioned stimuli as the date of the conditioned reflex formation (see Table 3).

Once formed, the conditioned reflex remains unstable, depending on the intra- and extra-exciters. Thus the records given in

TABLE 2

No. of experiment	Date	Age	No. of combinations	Action of unconditioned stimulus	Isolated action of conditioned stimulus	Conditioned reflex	Reaction	Support	Interval	Behavior in interval
			No. 1 V. C. Conditioned stimulus—yellow light.							
35	17/5	1 mo. 29 days	261	15″	5″	+	immediate sucking movements; open mouth	+	15″	calm voice sounds
			262	15″	5″	+	immediate clear sucking movements	+	10″	calm; looks up
			263	15″	5″	+	immediate sucking movements	+	10″	calm
			264	30″	5″	+	immediate sucking movements	+	20″	calm
			265	30″	5″	+	immediate sucking movements	+	20″	calm; whimpering
			266	30″	5″	+?	not clear hand and mouth	+	25″	calm
			267	30″	5″	+	immediate clear sucking movements	+	25″	calm
			No. 2 S. F. Conditioned stimulus—yellow light.							
32	29/5	1 mo. 29 days	234	15″	5″	+	open mouth	+	15″	calm
			235	15″	5″	+	immediately open mouth	+	20″	calm
			236	15″	5″	+	immediately open mouth	+	20″	calm; whimper
			237	15″	5″	+	immediately open mouth	+	25″	calm
			238	15″	5″	+	immediately open mouth	+	30″	calm

TABLE 2 (*Continued*)

No. of experiment	Date	Age	No. of combinations	Action of unconditioned stimulus	Isolated action of conditioned stimulus	Conditioned reflex	Reaction	Support	Interval	Behavior in interval
			No. 3 V. P. Conditioned stimulus—yellow light							
32	11/12	1 mo. 27 days	221	20″	5″	+	immediate sucking movement	+	20″	calm
			222	20″	5″	+	immediate sucking movement	+	30″	calm
			223	15″	5″	+	immediate sucking movement	+	15″	calm
			224	15″	5″	+	immediate sucking movement	+	20″	calm
			225	20″	5″	+	immediate sucking movements	+	15″	calm
			No. 4 G. P. Conditioned stimulus—yellow light							
18	19/3	1 mo. 28 days	128	30″	3″	+	intensification of sucking movements	+	25″	calm; sometimes moving
			129	30″	5″	+	immediate intensification of sucking movements	+	30″	stool
			130	30″	5″	—?	inhibition movement (stool)	+	25″	crying
			131	25″	3″	+	immediate inhibition and clear sucking movements	+	15″	calm; crying
			132	25″	3″	+	inhibition and clear sucking movements	+	20″	calm
			133	20″	5″	+	immediate inhibition and clear sucking movements	+	—	whimpering

TABLE 2 (*Continued*)

No. of experiment	Date	Age	No. of combinations	Action of unconditioned stimulus	Isolated action of conditioned stimulus	Conditioned reflex	Reaction	Support	Interval	Behavior in interval
			No. 5. E. F. Conditioned stimulus—yellow light							
7	23/3	2 mos. 1 day	39	30″	6″	+	sucking movements	+	30″	intensive crying
			40	30″	5″	⊕	inhibition of crying	+	30″	crying
			41	25″	5″	+	inhibition and sucking movements	+	20″	crying; looking up
			42	30″	3″	+	immediate inhibition and sucking movements	+	—	—
			43	30″	4″	+	immediate inhibition and sucking movements	+	—	—
			No. 6. N. T. Conditioned stimulus—red light.							
9	1/3	2 mos.	57	25″	3″	+	immediate sucking movements	+	3″	calm; whimper
			58	25″	3″	+	inhibition and sucking movements	+	1″	calm
			59	25″	4″	⊕	inhibition of movements	+	2″	calm; whimpering
			60	25″	4″	+	inhibition and sucking movements	+	50″	calm; whimpering
			61	15″	4″	+	immediate inhibition and sucking movements	+	25″	slight whimpering
			62	25″	5″	+	immediate inhibition; open mouth	+	30″	whimpering
			63	25″	5″	+	immediate inhibition and sucking movements	+	—	—

chart No. 4 (G.P.) showed an absence of any conditioned reflex, although a slight inhibition was present, expressed in a cessation of movements and shrieks. The disappearance of the conditioned reflexes in that case was due to the action of the bowels that occurred during the intervals between combinations 129 and 130. The same result was obtained in case No. 5 (E.F.) where an absence of the conditioned reflex owing to an extreme excitement from crying was noticed before combination No. 40. On the contrary, in the case of child No. 6 (N.T.) the conditioned reflex could not be formed owing to an extreme inhibition (calm during the intervals, turning head from side to side). Beside this, as we know from the general work on conditioned reflexes, any outside stimulus (knocks, noises, sudden light, cooling, odors) has a destructive influence on the already formed conditioned reflexes. In our later experiments, on strengthening the reflexes such influences did destroy the formed conditioned reflex.

The general results of the formation of the conditioned reflexes are given in Table 3.

TABLE 3

No.	Name	Age		No. of experiments	No. of combinations	Age at formation of conditioned reflex	
		Mos.	Days			Mos.	Days
1	V.C		14	34	260	1	29
2	S.F.		17	31	233	1	29
3	V.P.		17	31	220	1	27
4	G.P.	1	2	17	126	1	28
5	E.F.	1	15	6	38	2	1
6	N.T.	1	17	8	49	1	29

Note: No. 6 was born 1/1/30. The conditioned reflex was noticeable on 3/1. Since February has 28 days we consider that at the moment of the formation of the conditioned reflex the child was 1 month, 29 days old.

The age of the infants varied as can be seen from the table. Thus, the youngest was fourteen days old and the oldest 1 month 17 days. The final formation of the conditioned reflex occurred in all children within two to four days of the end of the second month of age.

IV. The Formation of Visual Differentiation of the Conditioned Reflexes

After we had been able to prove the stability of the conditioned reflex, giving reinforcement during several experiments, we were able to study the formation of the visual differentiation. Bekhterev and Chelovanov (6) have already pointed out the possible formation of differentiation and of a circumstantial conditioned reflex at the age of 121 days.

The process of the development of visual differentiation does not essentially differ from the formation of auditory differentiation, described in our paper mentioned above. The disappearance of sucking movements at the age of 4-5 months due to specific age characteristics, indicating the presence as well as the absence of differentiation, and the insufficient formation of a new indicator, opening of the mouth, proved to be rather embarassing for the study of differentiation in children at that age.

In one of the six children under experiment (E.F.), a child who showed a rather weak alimentary reaction, the sucking movements disappeared comparatively early. Therefore we can judge only approximately in his case as to the time of the formation of the differentiation. All the other children preserved sufficiently pronounced sucking movements to enable us to judge with a definite amount of certainty as to the development of differentiation.

The formation of the differentiation usually began soon after the strengthening of the conditioned reflexes. In only one case (No. 3, V.P.) was the differentiative signal introduced before the appearance of a stable conditioned reflex. The differentiation obtained determines the age limits within which a conditioned light reflex may usually be formed. In our present experiments a set of green colored electric bulbs placed symmetrically behind the experimental crib was used as a differential stimulus. At the beginning of the formation of the differentiation, a clear conditioned reaction answered the stimulus without reinforcement. One of the children responded with a somewhat different reaction, i.e., on the application of the differentiation stimulus he reacted by an inhibition of crying and movements. Extracts from experimental records are given in Table 4.

TABLE 4

No. of experiment	Date	Age	No. of combination	Action of unconditioned stimulus	Isolated action of conditioned stimulus	Conditioned reflex	Reaction	Support	Interval	Behavior in interval
23	28/3	2 mos. 8 days	189	15″	5″	+	immediate inhibition & sucking movements	+	15″	shrieks
			1 (Z)	—	15″	⊕	immediate inhibition; silent	—	15″	calm; whimpering
			190	15″	3″	+	immediate inhibition & clear sucking movements	+	—	—
24	29/3	2 mos. 9 days	196	20″	5″	+	immediate inhibition & clear sucking movements	+	15″	whimper
			2 (Z)	—	15″	⊕	inhibition; turning	—	10″	shrieks
			197	20″	3″	⊕	immediate inhibition	+	15″	crying
			198	15″	5″	⌟	immediation inhibition & sucking movements	+	—	—

Note: The letter "Z" near the number of the combination indicates the differentiation stimulus.

Our assumption that this child differentiated the green from the yellow light at once proved to be wrong. After the first three applications of the differential light with the result mentioned above the child, upon further experiment, reacted to the green light just the same as to the others, i.e., he generalized the differentiation and the conditioned stimuli. Such pseudo-differentiation is already known from the experiments on dogs.

Thus, as a rule, the process of differentiation in our experiments began with generalization. The differentiation stimulus—the green light—produced a pronounced conditioned reaction. As the differentiation stimulus was not followed by food reinforcement, the child became excited, turned from one side to the other, and did not react to the conditioned stimuli that followed the differential stimulus. But even in a comparatively quiet state the conditioned reflex, being generally unstable, sometimes died away.

Some extracts from our records are given in Table 5.

The extracts given in Table 5 show: (1) the absence of differentiation and a generalization of both signals; (2) in some cases (Nos. 2 and 4) the dying away of the conditioned reflex that followed the differentiation stimulus.

In its further development the reaction to the differentiation stimulus changed gradually. The sucking movements became less intensive, the latent period increased, and at last the child reacted to the green light by an inhibition of the general movements. During that period a rather vivid orientative reaction appeared. On giving the differentiation signal the child turned its head to the source of light, its general movements stopped for some time, as if the child were concentrating upon something, and then it either remained calm, whimpered, turned, or began to cry again. In the course of time the orientative action weakened and the child quickly and correctly answered to different light signals. The complete disappearance of the conditioned sucking movements and opening of the mouth in response to different stimuli, is considered to be the moment of the formation of the differentiation.

We give in Table 6 the records of our experiments showing where we could first state the presence of differentiation.

Differentiation as well as the conditioned reflex is unstable. When compared to the conditioned reflex the differentiation is even more

TABLE 5

No. of experiment	Date	Age	No. of combination	Action of unconditioned stimulus	Isolated action of conditioned stimulus	Support	Reaction	Support	Interval	Behavior in interval
No. 1	V.C.									
46	31/5	2 mos. 12 days	355	15″	5″	+	inhibition & clear sucking movements	+	15″	calm; whimpering
			3 (Z)	—	15″	+	immediate inhibition & sucking movements	—	10″	whimpering
			356	15″	5″	+	immediate inhibition & sucking movements	+	—	whimpering
No. 2	S.F.									
39	5/6	2 mos. 5 days	291	15″	5″	+—	immediate; open mouth	+	15″	calm
			6 (Z)	15″	—	+	open mouth several times	—	5″	shrieks
			292	15″	5″	⊕	inhibition; turning of head	+	15″	calm; weak whimpers
			293	15″	5″	+	immediate inhibition; open mouth 3 times	+		
No. 4	G.P.									
32	8/6	2 mos. 18 days	352	20″	3″	+	inhibition & sucking movements	+	20″	whimpering
			18 (Z)		15″	+	immediate shrieks & sucking movements	—	10″	shrieks
			353	35″	5″	—	loud shrieks	+	25″	turning; sucking movements; whimpering
			354	35″	5″	+	inhibition & sucking movements	+		
No. 6	N.T.									
14	9/3	2 mos. 8 days	81	25″	5″	+	immediate inhibition & sucking movements	+	40″	calm; whimpering
			3 (Z)		30″	+	inhibition & sucking movements	—	30″	calm; whimpering
			82	30″	4″	+	immediate inhibition & sucking movements	+		

TABLE 6

No. of experiment	Date	Age	No. of combination	Isolated action of conditioned stimulus	Conditioned reflex	Support	Reaction	Support	Interval	Behavior in interval
No. 1 V.C.										
58	14/6	2 mos. 26 days	458	15″	5″	+	immediate inhibition & sucking movements	+	10″	calm; weak whimper
			459	15″	5″	⊕	incomplete inhibition	+	10″	whimpering
			45(Z)		10″	—	slight inhibition & whimpering	—	10″	whimpering
			460	15″	5″	+	immediate inhibition & wide opening of mouth	+	10″	whimpering
			461	15″	3″	+	immediate; open mouth	+	15″	slight whimpering
			46(Z)		10″	—	slight inhibition & whimpers	—	2″	whimpering
			462	15″	5″	—	whimpers	+	10″	calm; slight whimpers
			463	15″	5″	+	immediate inhibition & clear sucking movements	+	10″	whimpers
			47(Z)		6″	—	very weak inhibition; whimpering	—	3″	whimpering
			464	15″	5″	⊕	clear inhibition	+	10″	whimpers
			465	15″	3″	+	inhibition & clear sucking movements	+	10″	whimpers
			48(Z)		10″	—	very weak inhibition; whimpers	—	5″	whimpers
			466	15″	5″	—	whimpers	+	10″	whimpers
			467	10″	5″	+	repeated opening of mouth	+		

TABLE 6 (*Continued*)

No. of experiment	Date	Age	No. of combination	Isolated action of conditioned stimulus	Conditioned reflex	Support	Reaction	Support	Interval	Behavior in interval
No. 2. S.F.										
63	6/7	3 mos. 6 days	460	15″	3″	+	immediate sucking movements	+	10″	calm
			112(Z)		15″	—	calm, whole time	—	10″	calm
			461	15″	3″	+	immediate distinct sucking movements	+	15″	calm
			113(Z)		10″	—	calm whole time	—	5″	calm
			462	15″	3″	+	immediate distinct sucking movements	+	15″	calm
			463	15″	3″	+	immediate distinct movements	+	15″	calm
			114(Z)		15″	—	quite calm	—	25″	calm; weak lip movements
			464	15″	5″	+	immediate sucking movements	+	30″	calm
			115(Z)		15″	—	quite calm	—	5″	calm
			465	15″	7″	—	calm	+	25″	calm
			466	15″	3″	+	immediate distinct sucking movements	+	15″	calm
			116(Z)		15″	—	quite calm	—	10″	calm
			467	15″	6″	+	immediate distinct sucking movements	+		
No. 3. V.P.										
76	7/2	3 mos. 23 days	375	35″	5″	+	immediate inhibition & sucking movements	+	30″	whimpers
			112(Z)		30″	—	slight inhibition & calm movements	—	1″	calm; whimpers
			376	30″	5″	+	immediate inhibition & sucking movements	+	30″	slight whimpers

TABLE 6 (*Continued*)

No. of experiment	Date	Age	No. of combination	Isolated action of conditioned stimulus	Conditioned reflex	Support	Reaction	Support	Interval	Behavior in interval
			113(Z)		30″	—	slight inhibition; whimpering	—	1″	whimpers
			114(Z)		45″	—	slight inhibition (2″) slight whimpering	—	3″	whimpers
			377	45″	6″	+	inhibition & sucking movements	+		
No. 4. 54	G.P. 8/5	3 mos. 19 days	442	15″	3″	+	immediate inhibition & clear sucking movements	+	15″	shrieks
			98(Z)		10″	—	continued shrieks	—	3″	shrieks
			443	15″	5″	+	inhibition; sucking movements	+	10″	crying; turning
			444	25″	3″	+	immediate inhibition & clear sucking movements	+	15″	whimpers; turns
			99(Z)		15″	—	shrieks; turns; looks toward light	—	3″	shrieks
			445	25″	5″	+	immediate inhibition & clear sucking movements	+	20″	shrieks; turns
			446	30″	5″	+	immediate inhibition & clear sucking movements	+	15″	whimpers
			100(Z)		10″	—	whimpers; looks toward light	—	2″	whimpers
			447	30″	5″	+	inhibition & distinct sucking movements	+	15″	shrieks
			101(Z)		15″	—	very loud shrieks	—	5″	shrieks
			448	30″	3″	+	immediate distinct sucking movements	+		

TABLE 6 (*Continued*)

No. of expeirment	Date	Age	No. of combination	Isolated action of conditioned stimulus	Conditioned reflex	Support	Reaction	Support	Interval	Behavior in interval
No. 5. E.F.										
32	4/5	3 mos. 12 days	191	20″	5″	+	immediate inhibition & sucking movements	+	15″	shrieks
			16(Z)		20″	—?	inhibition & weak sucking movements; shrieks	—	30″	shrieks
			192	25″	5″	+	immediate inhibition & clear sucking movements	+	35″	shrieks
			17(Z)		25″	—?	inhibition and sucking movements	—	35″	shrieks
			195	25″	5″	+	immediate inhibition & sucking movements	+		
No. 6. N.T.										
39	12/4	3 mos. 12 days	205	25″	6″	+	immediate inhibition & sucking movements	+	30″	whimpers
			206	20″	5″		immediate inhibition & sucking movements	+	20″	whimpers
			70(Z)		30″	—	inhibition; whimpering	—	60″	calm; whimpers
			207	25″	5″	+	immediate inhibition & sucking movements	+	25″	whimpers
			71(Z)		30″	—	some inhibition; whimpering	—	60″	whimpers
			208	15″	4″	+	immediate inhibition & clear sucking movements	+	60″	shrieks
			72(Z)		25″	—	inhibition; looks toward light	—	30″	whimpers
			209	20″	4″	+	immediate inhibition & clear sucking movements	+		

susceptible to change. Sometimes the production of a somewhat higher state of excitation in the elementary center in a child is sufficient for the disappearance of differentiation. All the extra stimuli also destroy the differentiation present, and even in the case of an established differentiation the stimulus, used for the first time on the experimental day, sometimes gives a conditioned reaction. With some children a very firm differentiation acquires in its development a characteristic "reciprocal reaction," i.e., the child can answer to the differentiation stimulus by a sharp intensification of crying. The crying weakens as soon as the signal disappears. In the records given (Table 6) one of the children (No. 4, G.P.) answered with an intensification of crying in applications 99, 100, and 101 of the green light. In the intervals between stimulations this crying weakens to a considerable extent.

In all children under experiment it was possible to form differentiation by a certain age. General results are given in Table 7.

TABLE 7

Number	Name	Age at formation of the differentiation		Date	Age at appearance of the differentiation		Date	Number of differential stimuli given
		Mos.	Days		Mos.	Days		
1	V.C.	2	12	31/5	2	26	14/6	44
2	S.F.	2	2	4/6	3	6	6/7	111
3	V.P.	1	23	7/12	3	23	7/2	111
4	G.P.	2	8	28/7	3	19	8/5	96
5	E.F.	3	5	27/4	3	12	4/5	14*
6	N.T.	2	7	8/3	3	12	12/4	69

*The given differentiation was not clear.

The analysis of the material presented on the formation of the visual differentiation allows us to distinguish three definite stages in the process of the formation of the differentiation. These stages are as if "reciprocal" to those that we see in the formation of the conditioned reflex. The differential stage of the yellow light from the green precedes the generalization stage, indicating

the generalization of the conditioned reflex in the great cerebral hemispheres. This stage may be called the first stage in the development of the differentiation.

The weakening of the conditioned reaction, the increase of the period of the differentiation signal, and at last a pronounced inhibition of general movements, together with the disappearance of the sucking movements, may be considered as the second stage.

A clear differentiation of the conditioned and differentiative stimuli forms the third stage. With some children the complication of the differentiation by a negative reaction may be considered as an additional fourth and last stage.

The presence of the third stage may be considered as the final step in the formation of the visual differentiation. In conclusion, we should mention that, in the study of the formation of the conditioned reflexes and their differentiation to the light stimuli, the question of color discrimination in infants has not been the aim of our study. The analysis of some of the simpler nervous processes in the formation of the conditioned reflex in infants was the task of the present investigation.

V. Summary

1. Conditioned reflexes in response to light stimuli are formed in infants by the end of the second month of their life.

2. There are three stages in the formation of conditioned reflexes:

a. the absence of any reaction,

b. the beginning of inhibition and the appearance of the first signs of a conditioned reflex,

c. the stage of a stable conditioned reflex.

3. The time of formation does not depend on the number of combinations of the conditioned and unconditioned stimuli, but is determined rather by the degree of maturity of the parts of the great cerebral hemispheres involved in the responses.

4. In most infants (five out of six) the first visual differentiation of the light stimuli is formed in the fourth month of their life.

5. In its development visual differentiation passes through all the stages of conditioned-reflex formation, but in reverse order.

6. The differentiation formed is very unstable and is easily disinhibited.

7. The presence of visual differentiation marks the beginning of the function of the great cerebral hemispheres.

8. The appearance of the third stage in the development of the conditioned reflex is to be considered as the date of the conditioned-reflex formation.

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SHORT ARTICLES AND NOTES

THE STANDARDIZATION OF SIXTY-CYCLE ELECTRIC SHOCK FOR PRACTICAL USE IN PSYCHOLOGICAL EXPERIMENTATION

T. W. Forbes and Arthur L. Bernstein[1]

A great deal of interest has recently been shown in the problem of obtaining a psychologically constant electric shock for use in various types of psychological experimentation, both animal and human (5, 6, 7). The problem has been raised most directly in relation to punishment grilles for animal work. Jackson and Riess,[2] working with the Jenkins electrical stimulation system, have pointed out that the possibility of obtaining a stronger or a weaker shock by placing a smaller or larger skin area upon the grille must be eliminated (2). They have accordingly investigated the relationship between the current value necessary to stimulate and the area of the stimulating electrode, and have made certain suggestions based on their results. We have repeated essentially the same type of investigation but have measured one further electrical quantity, viz., voltage drop across the tissue, and feel that our additional results alter to some extent the suggestions to be made.

The ordinary 60-cycle alternating current is probably for most experimenters the handiest source of supply and for this reason a method of standardizing this particular frequency should be of great practical importance. Such standardization must take into account different degrees of moisture of the skin due to different degrees of activity of the sweat glands. It has been shown that there is a change of skin resistance not only with visible perspiration but also with insensible perspiration (1). Such resistance changes also occur on electrical stimulation so that whether or not temperature factors are controlled there still will be moisture changes due to varying activity of the sweat glands.

Certain incidental remarks in papers on the galvanic skin response, together with data from studies using electrical shock for conditioning and other purposes, indicate that stimulation occurs at widely different current intensities when the general conditions of electrode size, etc., are approximately the same, an observation which suggests that the voltage drop across

[1]The writers desire to express their appreciation of the assistance of Mr. Harvey Stoll in the work of treating the data.

[2]We are indebted to the authors for the opportunity of reading the manuscript of their article.

the skin may be a determining factor, as well as current strength. Our present study indicates that such is the case.

Method

The procedure was similar to that of the previous investigators (2) except that we employed essentially a method of limits. The strength of the applied current was gradually increased until the subject reported that he felt stimulation. The measures obtained were, therefore, essentially those yielded by the ascending series of the classical method of limits. Two electrodes of equal area were used, the sizes of these being given in Table 1.

TABLE 1
Diameters and Areas of Electrodes

Electrode number	Diameter (mm.)	Area (sq. mm.)
1	not used	not used
2	.14	.015
3	.35	.096
4	.51	.20
5	1.30	1.33
6	2.58	5.24
7	4.0	12.56
8	6.0	28.22
9	8.0	50.25
10	10.0	78.50

Twenty determinations per area were made on each subject on each of four separate days.

The subject was instructed to place the middle finger of the right hand on one of the pairs of the electrodes and to report as soon as he felt a shock. At this point readings of the milliammeter and voltmeter were taken. Even numbered areas were taken in ascending order, followed by odd numbered areas in descending order. To obtain a "dry" skin, the finger was placed on a towel between readings and any possible perspiration was wiped from the electrode board,[3] while to obtain a "moist" skin the finger was immersed in physiological saline solution and dried on a blotter before each reading. The experimenter and apparatus were screened from the view of the subject.

The electrical circuit is shown in Figure 1. The hook-up provided a continuously variable secondary voltage of from zero to 750 volts and the 20,000-ohm resistor kept the secondary circuit constantly closed in order to avoid possible surges in the secondary at make or break of the electrode

[3]This method of obtaining dry skin is the same as that employed by Jackson & Riess.

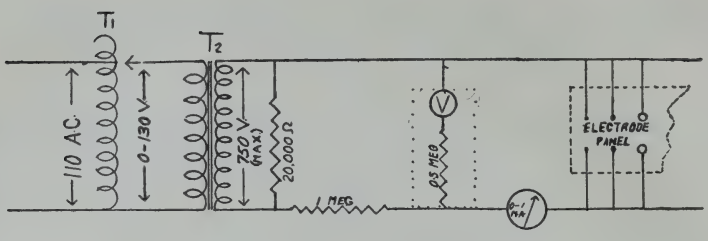

FIGURE 1

CIRCUIT ARRANGEMENT

T_1 = variable auto-transformer, T_2 = voltage step-up transformer
V = copper oxide rectifying microammeter used as high resistance voltmeter
MA = copper oxide milliammeter

circuit. A 1-megohm resistor in series with the electrodes served as a current limiter, while voltage readings across the skin itself (electrode to electrode) were taken on a high resistance voltmeter (V) consisting of a 0.5-megohm resistor and a Weston 0-500 microammeter. Calibration was checked against a commercial A.C. voltmeter. A Weston 0-1 copper oxide rectifying milliammeter (MA) gave the average current through the electrodes.

RESULTS

As shown by Figures 2A and 2C, the current values necessary to stimulate at each of the different areas with the skin dry followed closely those obtained by Jackson and Riess. Figure 2A is plotted on linear axes to show in most easily recognizable form the relation between the current to stimulate and the area of the electrode. The same curves are replotted in Figure 2C in which both axes are logarithmic. The straight line relationship shown by this figure indicates that current varied roughly as the fourth root of the area.

Curves for the two conditions of moisture for each subject are shown in Figures 2B and 2C for voltage and current respectively. From these it will be seen that when the skin is moist greater current is necessary to produce threshold stimulation and, at the same time, a lower voltage suffices. It has been suggested that, since the skin is moist, the effect is merely that produced by a spreading of the current, giving in effect a stimulated area larger than the electrode. If this were the case, impedance should give a rough index of the increased effective electrode area since, for impedances in parallel, the total impedance will reduce by 1/2, 1/3, etc. as equal increments of area are added. Approximate impedance was therefore figured from each pair of voltage and current values. Such computa-

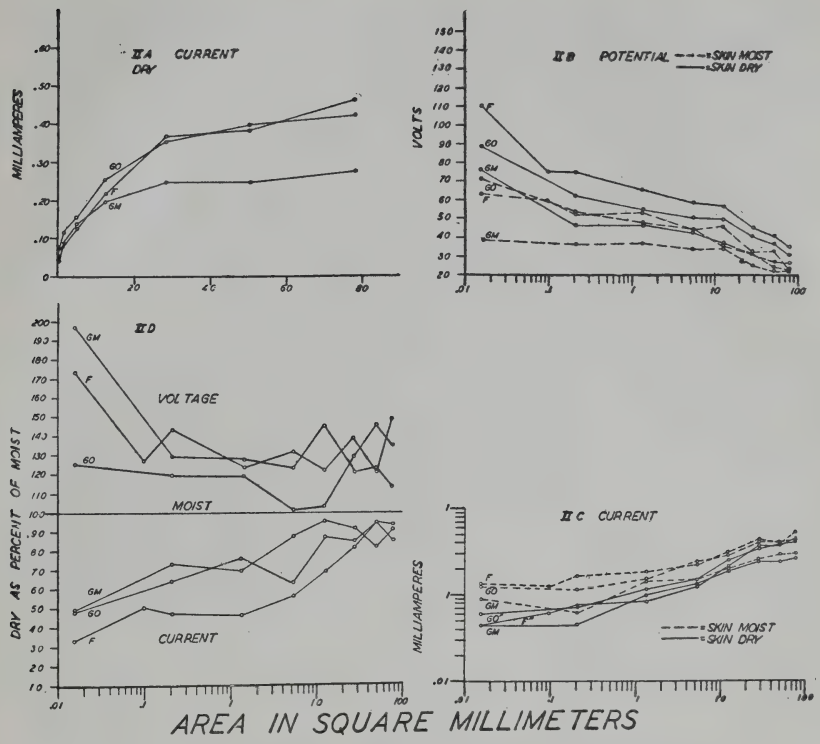

FIGURE 2

VARIATIONS OF ELECTRICAL QUANTITIES WITH MOISTNESS OF THE SKIN FOR THE VARIOUS AREAS

A = Current necessary to stimulate with dry skin. Linear plotting.
B = Applied potential at electrodes for threshold stimulation. Areas plotted logarithmically.
C = Current for threshold stimulation. Curves of Figure 2A replotted with both axes logarithmic, and corresponding curves for wet skin added.
D = Variations due to moistness of skin shown as percentages.

tions of tissue impedance, with correction for current strength, indicated that this spread would have to be six times that of the electrode area to account for the difference at small areas and roughly twice the extent of the area to account for it at the largest areas. It does not seem, therefore, that such spread accounts for the difference in current and voltage to stimulate when the skin is moistened.

Since in curves A, B, and C voltage and current are shown on different scales, the relationship between the values necessary to stimulate in the case of dry and in the case of moist skin are shown as *percentages* in Figure 2D. The value obtained with wet skin was taken as 100 and the value to stimulate dry skin was expressed as a percentage of this value. Again the fact is clear that current necessary to stimulate differed in the two conditions as much as 50% for the small areas and less for the larger electrodes. Similarly voltages varied to a greater extent at the smaller areas. However, whereas only 50 per cent of the current necessary on moist skin stimulated dry skin, the voltage across the skin was 80 per cent *greater* in the latter case. It should perhaps be repeated that each curve represents 40 determinations per area per skin condition, 10 per day for 4 days.

It should be pointed out, for the benefit of those who have not investigated the physical relations involved, that maintaining a constant current intensity through a given area of skin does not necessitate a constant voltage across the skin. Conversely, a constant applied potential across the skin does not guarantee a constant current. The latter is, perhaps, a commonplace since it is apparent that changes of skin resistance will affect current. The former will be easily seen if it is recalled that voltage may be expressed in terms of current and resistance or impedance for D.C. and A.C. respectively, thus $E = IR$, and $E = IZ$, where E is potential, I is current, R is resistance and Z is impedance. Now if I is held constant, E will vary as Z (or R) varies.

Our results above show that, under the conditions met with in animal punishment grilles, neither voltage nor current value alone is a satisfactory index of the sensory effect of shock. The results *do* suggest that power *may* be such an index.

Areas 7 and smaller usually gave punctiform pain and areas 8 and larger a more diffuse, vibratory shock sensation. Possibly the summation from the stimulation of a larger number of receptors by the larger areas is involved in the latter, which is probably similar to the slight "tactual" sensation mentioned by others (2, p. 264).

Discussion

Our technique was simpler and less time-consuming than a more conventional psychophysical method, and since our results are similar to those obtained by a more tedious method, we believe ours has certain advantages.

Since roughly $E \times I = P$ where E is voltage, I is current and P is power, if power necessary to stimulate were constant, then E should decrease as I increases, and *vice versa*. It will be seen that curves 2B and 2C do show this type of relationship.

Since our set-up gave no measure of effective power expended upon the

tissue, measurements of effective power taking into account time relations between current and voltage should be made. It is worth noting, however, that the two quantities used in chronaxie measurements are related to the energy of the stimulating circuit.[4] It should also be pointed out that both applied voltage and current are involved in the rate of change of current found to be significant in D.C. shock by Muenzinger & Walz (5), and that power in our A.C. circuit would correspond to the *variation* of power at make and break in D.C. stimulation.

Rough approximations indicate that increased power per unit area as well as current per unit area is necessary for stimulation with very small electrodes. This may correspond to a smaller number of end organs stimulated, a larger area giving greater sensitivity by summation.

The fact that current and voltage vary considerably with the smaller electrodes, and that both current and voltage (and therefore probably power) vary less as between the moist and dry skin condition with the larger electrode areas, leads to the following suggestions for practical use in sensory stimulation: (1) In case constant current is used an electrode of large area, (at least 1 cm. diameter) making contact through a moist pad, should be attached to the skin for the duration of the experimental period; or (2) if very small areas are used, as suggested by Jackson and Riess, control of power rather than current should be tried. Also some means of eliminating all other electrodes as soon as the circuit is made through any two should be provided. The elimination of the remaining electrodes from the active circuit is suggested to avoid the obtaining of a larger total effective area by summation. If constant current *is* used, the animal's feet should be kept in a fairly constant moist state.[5] With the larger electrodes, the variation in shock to be expected when current is held constant is less in proportion, but the use of constant energy would probably be an improvement even here. If the electrodes are attached to an animal, the technique described by Light and Chaffee (4) of passing an animal through a magnetic field could be employed to eliminate troublesome trailing wires. The animal carries a coil which picks up the magnetically transmitted energy and transforms it into current. A high resistance secondary coil might be used to provide the current limiting feature.

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[4]We wish to acknowledge helpful suggestions in this connection by Dr. H. H. Jasper of Bradley Home and Brown University (3). For a simple presentation of chronaxie relations see (3, p. 126.)

[5]This suggestion is made by Dr. T. A. Jackson, who reports that he has previously obtained satisfactory results by periodically moistening with a sponge the floor preceding the grille. Obviously no excess water or saline solution is permissible and the grille must be constructed to prevent short circuiting by stray electrolyte.

SUMMARY

1. Using 60-cycle A.C., we have corroborated previous experimenters in showing that the total current necessary for threshold stimulation increases logarithmically with electrode area. On the other hand, the necessary current *density* decreases with area.

2. The current necessary to produce threshold stimulations in humans increases while voltage decreases when the moistness of the skin increases for a given electrode area.

3. This suggests that power (energy) is the significant physical variable and that standardization should therefore be in terms of power rather than of current. Such a conclusion is reasonable when considered in relation to chronaxie quantities.

4. Measures of actual effective power should be made since we had no measure of current and voltage phase relations.

5. Moistness of the skin affects the threshold values to a larger extent with very small electrodes. Our data suggest the use of fairly large electrodes (80 sq. mm.) attached to the leg of the animal, or else if a grille consisting of small areas is used that standardization in terms of effective power should be tried. In case standard current is used with very small electrodes, an attempt to control moisture should be made (See Note 5).

6. The smaller areas (12 sq. mm. or less) usually gave a sharp, punctiform pain sensation, while the larger areas gave a more diffuse pain sensation. Greater current, voltage, and power per unit area were necessary for stimulation at the smaller electrodes. It is suggested that there may be a relationship to the number of end-organs stimulated, the larger areas giving greater sensitivity by summation.

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A CRITICAL NOTE ON CERTAIN EXPERIMENTAL WORK ON THE CONDITIONED RESPONSE

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Probably no other recent field of psychological endeavor has undergone a greater acceleration than that concerned with the phenomenon of "conditioning." Not only has the conditioned reaction been increasingly utilized as a *method of testing the receptor and reactor capacities of animals,* but many of the studies have made attempts at the *experimental investigation of the nature and mechanism of the conditioning process itself,* with the attendant theoretical implications for a natural, systematic science of organismic behavior. Starting out in physiology as a simplified study of certain digestive functions and in psychology as a laboratory study of a simplified learning situation, conditioned-response experimentation has traversed almost the entire gamut of physiological and psychological activities, including experimental conditioning of the patellar, plantar, abdominal, and Achilles reflexes; the reflex withdrawal from a painful stimulus; the reflex wink; pupillary or iridic reflex; variation in the amplitude and rhythm of respiration and of the pulse; vasomotor constriction; rise in pitch of the voice; fetal kicking reactions; food-taking reactions of newborn infants and older children; manual, locomotor, and other gross "molar" reactions; gastric secretion; salivation; regurgitation; defecation and micturition; diuresis; and immunology or defensive blood reactions, such as aglutination, anaphylaxis, digestive leucocytosis, and antitoxin and antibody formation. Such experimentation has invaded the more complex fields of perception, learning, motivation, emotion, cognition, abnormal behavior, and, as illustrated by the recent work of Hudgins, even voluntary action. So great are the possibilities of such research that future developments can be only speculatively inferred.

The *methodology* of conditioned-response experimentation has often been pointed to as an example of rigorous scientific procedure. The wide variety of *controls* utilized in the typical Russian investigations such as those of Pavlov and Bekhterev are well known by every psychologist. Especially constructed institutes for such experimentation have made possible controlled conditions comparable in every way with those utilized by the physicist in making his most precise measurements. The physiological condition and reactional history of the experimental subjects are carefully controlled; the predetermined experimental stimuli are controlled; all extraneous stimuli of every modality are eliminated or controlled; the experimenter remains completely isolated from the experimental room; the stimuli are delivered automatically; and the reactions are registered objectively. Most of the Russian work has been conducted on *dogs,* although occasionally protozoa, crustacea, cephalopoda, chaetopoda, urochorda, fish,

frogs, reptiles, pigeons, chickens, guinea-pigs, rats, cats, sheep, and horses have been employed. Rarely, however, has an infrahuman primate been used as subject, and most of the work on humans has been done on infants and children.

It is the writer's contention that many recent American investigations of the conditioned response, in contrast to the typical Russian study, can be criticized on two grounds: *First,* such studies have frequently overlooked the extrinsic experimental controls which are so necessary for complete isolation of the experimental subject from all external stimuli other than the predetermined ones which are experimentally administered. The mere presence, for example, of the experimenter or another subject in the same room is enough to invalidate completely any exact data which might be so obtained. *Second,* and this objection is more often overlooked due to its subtle and intrinsic character, experimental work on conditioning in complex organisms, such as the monkey, ape, and particularly the human, is often completely vitiated by the fact that certain implicit reactions, with their attendant stimulations, are frequently occurring simultaneously or temporally adjacent to the predetermined stimulations experimentally administered by the investigator. Any human subject in such a study, for example, will frequently report that certain "mental sets," "thoughts," or "ideas" occurred to him in the course of the experiment, and it would be quite absurd blithely to deny the existence or stimulational value of such symbolic activities. Elimination of such implicit behavior in the human is virtually impossible. It is true, no doubt, that such processes occur to a certain extent in the lower animals, although their operation in such cases is small indeed in comparison to the complex linguistic behavior of man.

The above fact may well account for differences in experimental results which have from time to time been found between human and infrahuman subjects. It should be kept clearly in mind that the writer by no means wishes to deny or even question the fact that such complex organisms as man become conditioned, but rather to emphasize the fact that in such cases it is difficult to isolate and control the stimulational situation so that valid data may be obtained regarding the nature and basic mechanism of the underlying process. At present it would seem that such data must be obtained on organisms of less complex phylogenetic and behavioral status.

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APPARATUS

APPARATUS AND TECHNIQUE FOR GUSTATORY EXPERIMENTATION

CARL PFAFFMANN

In connection with a series of experiments with human subjects, the following apparatus was found to be well suited for the determination of threshold values in gustation.

The apparatus consists of a thermostatically controlled water-bath from which solutions can be directed through a glass applicator placed on the tongue. This applicator (Figure 1), a modification of that used by Hahn

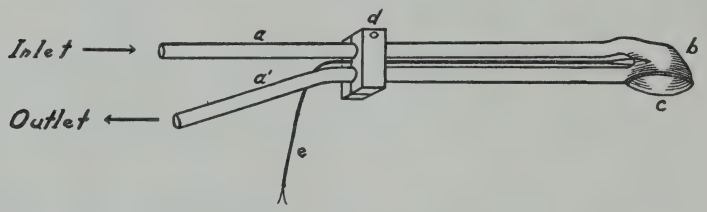

FIGURE 1

and Günther (1), consists of two parallel tubes (*a* and *a'*) 7 mm. in diameter, fused into an open semi-spherical bulb (*b*) at one end, the aperture (*c*) of which is 1.5 cm. in diameter. A thermocouple junction (*e*) of Chromel-Alumel is sealed into the bulb. The upper tube (inlet) connects by means of rubber hosing and a three-way stop-cock to two funnels immersed in the water bath; the lower tube (outlet) connects with a drain and the whole structure is reinforced by a small wooden clamp (*d*).

As an example of the general method, the procedure used during an experiment now being conducted will be described. Trials are carried out at least two hours after eating. At the beginning of an experimental session the subject first rinses the mouth with distilled water and then seats himself before the apparatus, resting the head against a support attached to the chair. The applicator is mounted on an adjustable rod directly in front of the subject's mouth so that, by a slight extension of the tongue and movement of the head, he is able to place the tongue against the aperture of the applicator. A small mirror before the subject serves to guide him in placing approximately the same area of the tongue (anterior mid-region) under the applicator for each stimulation.

By means of pipettes, the experimenter transfers 150 cc. of distilled water and 30 cc. of salt solution from bottles immersed in the bath to the

two funnels. At a ready signal, the tongue is placed lightly against the applicator and distilled water flowed through the system for fifteen seconds. This serves to rinse the surface of the tongue and to adapt it to the given temperature. The adapting solution is immediately followed by salt solution for five seconds, which in turn is followed by distilled water to eliminate the salt. Rapid manipulation of the stopcock makes it possible to change from distilled water to the solution with a minimum of disturbance in the continuous flow of liquid. At least thirty seconds elapse between stimulations during which interval the experimenter refills the funnels. Controls, such as flushing the solution funnel with the solution to be used before each refilling and use of individual pipettes for each of the solution concentrations, are maintained. The maximum number of single stimulations (Constant Method) never exceeds fifty in one experimental session.

Since temperature is known to have a definite effect upon gustatory sensitivity, the apparatus and general method described above seem superior to any previous method employed in experiments on taste. Measurement of the temperature directly at the surface of the tongue by means of the thermocouple has indicated that under the present conditions the variation in temperature, arising from extraneous conditions, does not exceed $\pm.15°$ C. For a temperature of 33° C. the results for three subjects indicate that the RL value for sodium chloride is approximately 3/1000 of a molar concentration. Furthermore, the other conditions of stimulation, namely time and area, are controlled. Salivary dilution of stimulating solution and incidental stimulation of the olfactory receptor is prevented.

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AN ELECTRONIC VOICE-KEY

Leo D. Kahn

The apparatus herein described is a highly sensitive voice-actuated relay. It is built around one of the recent developments of electrical engineering, the thyratron, and is superior in many respects to the conventional voice- and lip-keys. This device can find many applications in the psychology laboratory whenever it is necessary to start or stop apparatus by means of the human voice or other sound source; for example, in the determination of the time required for verbal reaction to stimuli of various sorts. This apparatus was developed at the Psychology Laboratory of the Boston Psycho-

pathic Hospital, and is at present being utilized in measuring the time for verbal response to visually presented material.

It has been customary to record the voice-impulse by means of a voice-key consisting primarily of a foil diaphragm which reacts against a metal contact, thus closing or opening an electric circuit when sound strikes it. The drawbacks of such a device are many. It must be placed close to the mouth of the subject who must speak well above his natural volume of speech. It is unlikely to function at every voice-impulse, since a definite pressure must be established between diaphragm and contact to avoid excessive electrical resistance. Further complications are introduced by the contacts opening and closing several times at a given impulse because of the natural vibration of the diaphragm. Lip-keys eliminate contact failure but introduce discomfort to the subject which may influence his spoken reaction.

These shortcomings are avoided by using the electronic relay here described. It will function even when the subject speaks in a whisper five feet from the apparatus. Nothing in the apparatus will deter the subject from giving a natural response since it does not inconvenience him in any way, and may be kept entirely out of his sight. Discrepancies due to apparatus characteristics are reduced to a minimum, the total lag being well under a thousandth of a second. As high a degree of reliability and precision as is usually desired is insured. The entire unit can be constructed in the ordinary workshop by any person with a fair knowledge of electrical practice, and at an expense somewhat below that of the conventional voice-key.

The accompanying diagram shows the schematic layout of the apparatus. It consists essentially of a loud-speaker employed as a microphone feeding into a Loftin-White amplifier, which in turn acts by means of a thyratron upon the load. The thyratron is fundamentally a three-element vacuum-tube which contains a small amount of mercury vapor. The current source is 110-volt A.C., which is applied through two separate transformer, rectifier, and filter circuits.

Under normal quiet conditions with the amplifier turned on, no current will flow through the load. However, when sound energy passes through the amplifier the voltage drop across R_6, R_7, and L_1 decreases. This lowers the negative potential at the thyratron grid, causes ionization of the gas within the thyratron, and consequently allows current to flow through the load. This current will continue to flow until the thyratron-plate circuit is opened, thus stopping the ionization. If the plate circuit is again closed this process may be repeated.

The parts may be mounted in any suitable fashion with the loud-speaker at any distance desired. R_3 serves to stabilize the amplifier and may be

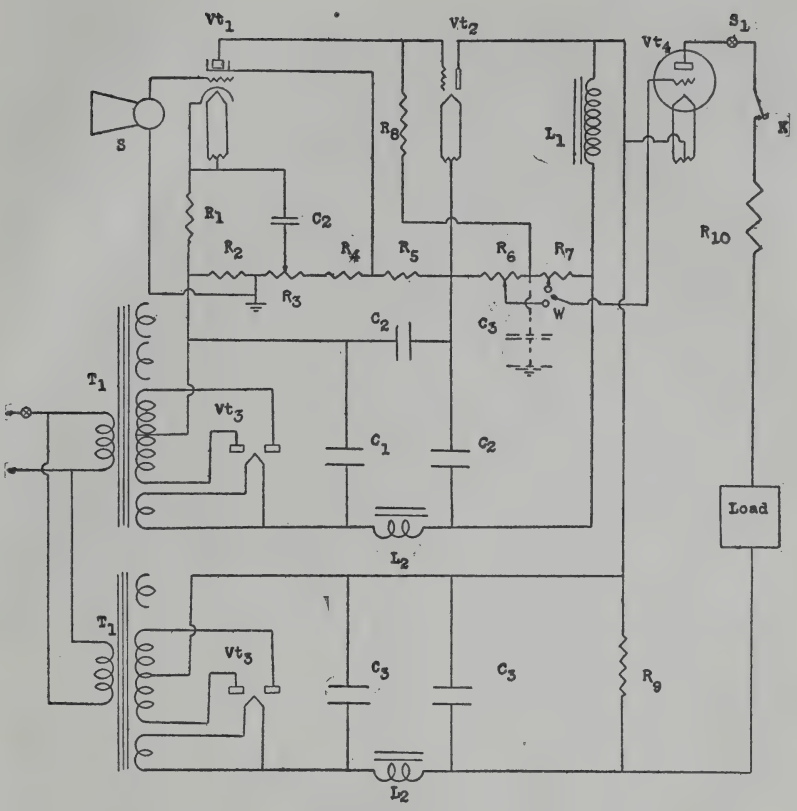

FIGURE 1

Vt_1 is a type 24 Radio Vacuum Tube. Vt_3 is a type 80 Radio Vacuum Tube.
Vt_2 is a type 45 Radio Vacuum Tube. Vt_4 is a type FG-17 Thyratron.
S is an Exponential Horn Loud-speaker 9 ft. in length.
T_1 is a Power Transformer with a 110-volt Primary, two 2.5-volt Secondaries, one 5-volt Secondary, and one 700-volt Secondary which is center tapped.
L_1 is an Audio Frequency choke. L_2 is a 30 Henry Filter choke.
Resistance values:

R_1 is 50,000 Ohms @ 2 Watts.
R_2 is 425 Ohms @ 2 Watts.
R_3 is 200 Ohms @ 2 Watts.
R_4 is 775 Ohms @ 2 Watts.
R_5 is 4,700 Ohms @ 2 Watts.
R_6 is 25,000 Ohms @ 2 Watts.
R_7 is 0.1 Meg. @ 2 Watts.
R_8 is 0.5 Meg. @ 2 Watts.
R_9 is 40,000 Ohms @ 10 Watts.

K is a Key that opens the circuit when depressed.
W is a Single-point, Double-throw Switch.
Condenser values:

C_1 is 8 Mfd. @ 500 Volts.
C_2 is 1 Mfd. @ 500 Volts.
C_3 is 0,005 Mfd.

adjusted by connecting a pair of head-phones across L_1 and setting its arm to the point of minimum hum. C_3 is intended to eliminate the effects of external electrical disturbances and may not be necessary under favorable conditions. The combination of R_6, R_7, and W regulates the sensitivity of the device, i.e., the minimum volume level of sound necessary to actuate the load. In this way the apparatus may be set to react to the desired sound intensity only and not be disturbed by incidental noises, such as those produced by people moving about, etc. K is a key that opens the circuit when depressed. R_{10} is a ballast resistance which protects the load from excessive current and must be determined for each specific case.

To set the apparatus in operation the line switch is closed while S_1 is left open. After a lapse of several seconds S_1 is closed. With W making contact to the arm of R_6 the latter is adjusted to the desired sensitivity. Sensitivity increases as the arm is turned toward the positive side. Should the thyratron ionize with no sound impulse entering the amplifier R_6 should be turned toward the negative side just enough to remedy this condition. As the thyratron ages it will be necessary to turn the arm of R_6 further toward the positive side for maximum sensitivity, and finally W will have to be switched to R_7 and this unit manipulated in a similar fashion. With these adjustments properly made a sound impulse will cause current to flow through the load. Depressing and releasing K will stop the current and set the apparatus for the next impulse.

The usual precautions regarding the handling and application of the thyratron should be observed.

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A NEW SPECTRAL COLOR APPARATUS FOR STUDIES OF PRIMATE COLOR VISION[1]

WALTER F. GRETHER

I. INTRODUCTION

Psychologists, physiologists, and physicists have devoted a great amount of attention to the experimental study of the phenomena of color vision. Although these phenomena have been thoroughly investigated and described for human beings, the field has been only partially explored for subhuman primates because of the methodological difficulties involved.

The available data suggest that the development of color vision within

[1]This research was carried out at the University of Wisconsin, and was financed in part by the research funds of that institution for 1932-1933.

the mammalian order may follow an evolutionary course. No investigator, using adequate controls, has proven the existence of color vision in any *mammal* outside the primate order. For the lowest existing primates, the lemurs, Bierens de Haan (2) has shown that color vision is either absent or very weak. Watson (9), Bierens de Haan (1), Trendelenberg and Schmidt (8), and Klüver (5) have found that both New and Old World monkeys are capable of color discrimination. Trendelenberg and Schmidt have adequately demonstrated that rhesus and Java monkeys have trichromatic vision similar to that of the normal human being.

These data suggest that important stages in the development of color vision have occurred between the lemurs and the monkeys and that the careful study of existing forms would offer interesting material on this problem. The following specific questions indicate points of attack in the elucidation of this problem of evolutionary development.

1. In which members of the primate series did the capacity for chromatic vision first develop?

2. Was the development of color vision within the primate order progressive or saltatory?

3. Did improvement manifest itself throughout the entire spectral range or did sensitivity to some specific color or colors first develop, as suggested by the Ladd-Franklin theory?

4. Studies of rodents and cats show that these animals are sensitive to a shorter range of wave-lengths than are human beings. What variations in length of the visible spectrum exist in the primate order?

Subhuman primates may be used appropriately not only in investigating the evolution of color vision but also in determining the neurological-physiological correlates of color vision. The similarity of the visual system of the monkey to that of man and the convenient location of foveal representation in the approximate center of the occipital lobe in the monkey (4, 7) both suggest the possibilities of combining operative and behavior methods in answering the following questions:

1. How rigidly do the points on the retina which are sensitive to color correspond to points on the cortex?

2. Is the cortical area for blue-yellow more extensive than that for red-green color vision?

II. Methods of Presenting Color Stimuli to Animals

In the experiments on the color discrimination of animals, three types of color sources have been used, namely: colored surfaces, color filters, and the monochromator, an instrument producing spectral colors. Although the first two types are simple and inexpensive, they have many serious defects, as has been pointed out by Watson (9). For a comprehensive survey of all

aspects of color vision, for example, the determination of difference limens, the perception of color mixtures, and the length of visible spectra, the use of spectral colors is necessary. This fact was first appreciated by Watson (9), who devised a monochromator of the conventional type for use in his experiments on monkeys. Yerkes and Watson (11) later constructed another similar instrument which has been used by Lashley on the game cock (6), and by Watson and Watson on the rat (10). These monochromators produced two color patches by means of two slits which selected color from different parts of the same spectrum. The animal was trained to discriminate between the two colors presented simultaneously. With this system it is mechanically impossible progressively to equate the two colors for measuring small difference limens. Trendelenberg and Schmidt overcame this difficulty with their own monochromator by using a single color-patch, training the animal to respond only when the positive color was presented. They report, however, that this system of successive presentation is very tedious, requires two experimenters, and the animal has to be quite tame and specifically trained for the problem.

There are two reasons why monochromators have not been used more frequently by animal experimenters: the expense and difficulty that their

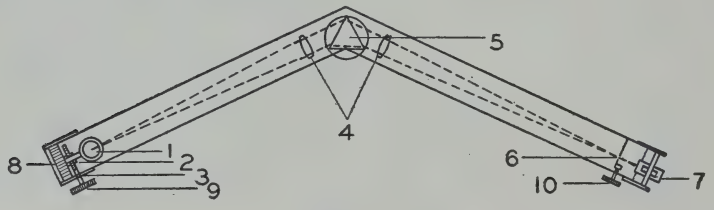

FIGURE 1

DIAGRAMMATIC TOP VIEW OF MONOCHROMATOR

1. Single-straight-filament lamp used as light source, and supported in asbestos-lined clamp.
2. Sliding block, supporting the lamp holder.
3. Micrometer screw controlling movement of sliding block.
4. Lenses, mounted on sliding supports (supports not shown).
5. Prism, mounted on adjustable table, and held firmly by a clamp (clamp not shown).
6. Slit for selecting color.
7. Mirror which reflects light downward to food boxes.
8. Linear scale which gives rough reading of the position of the sliding block and lamp.
9. Head of micrometer screw. See 3 above.
10. Micrometer screw controlling slit width.

All parts are mounted on a solid channel-iron frame. The total length is approximately 4 feet. The path of light is shown by the dash-lines in the figure.

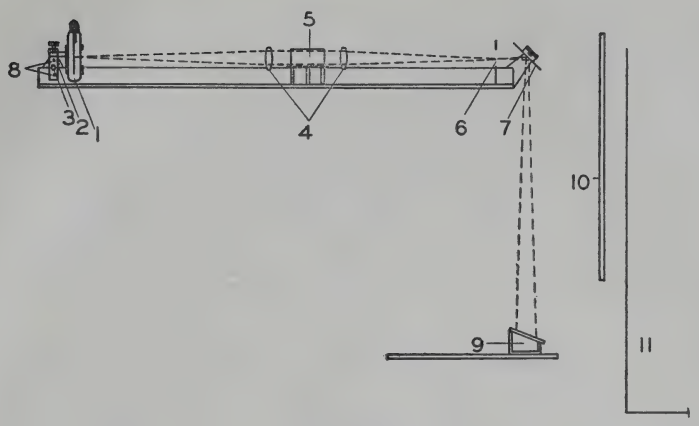

FIGURE 2

DIAGRAMMATIC SIDE VIEW OF MONOCHROMATOR

1. Lamp.
2. Sliding block.
3. Micrometer screw controlling movement of block.
4. Lenses.
5. Prism.
6. Slit.
7. Mirror.
8. Rods on which block (2 in Figure 2) slides.
9. One of the two food boxes mounted on a slide.
10. Opaque sliding screen.
11. Animal cage.

construction entails; and the complications added to the experiment by the complexity and delicacy of the instrument.

The author, desiring to carry on some work on color vision, took up the problem of building a simpler and less expensive type of monochromator than the previous ones. With the cooperation of Dr. J. E. Mack of the Department of Physics and Dr. H. F. Harlow of the Department of Psychology of the University of Wisconsin a monochromator was designed and two instruments built according to the plans.

III. A SIMPLIFIED MONOCHROMATOR

Figures 1 and 2 show the general plan of construction, leaving out many of the details. [Complete specifications are on file in the Physics Library of the University of Wisconsin (3)]. The novel feature of this monochromator is the use of a single-filament lamp as a substitute for the usual source slit. The innovation makes unnecessary the use of an arc lamp

(or other intense light source), a condensing lens, and a source slit. This saving of parts not only reduces the expense considerably,[2] but also makes it possible to build a simpler, more compact unit, in which adjustments for color and brightness are simple and accurate. Furthermore, it made possible the construction of two independent instruments, thereby simplifying the experimental procedure, especially that for securing difference limens.

Adjustment for wave-length is secured by lateral motion of the source lamp, which is mounted on a sliding block. A micrometer screw controls the latter. With the linear scale behind the block, and the graduations on the screw head, the position of the lamp can be read in 2000ths of an inch. By the aid of a calibration curve, these readings are translated into mean wave-length.

Variations in brightness are made by changing the width of the exit slit (No. 6 in Figure 1). The width is also controlled by a micrometer screw. A change in brightness is accompanied by a slight change in purity, and since only one edge of the slit moves, it also affects the average wave-length very slightly. With the aid of a curve, corrections can be made for the latter. The former is too slight to be serious, since in determinations of color limens the brightnesses, and hence the slit widths, are made equal. Brightness changes can also be made by varying the voltage applied to the lamp. An intensity of about .15 candle power was ordinarily used in actual experimentation.

In order to make sure that the color produced by the monochromator was pure enough to use for color-vision tests, a detailed analysis was made. For this the light was sent through a calibrated Hilger spectrometer. For the slit width later used in actual experimentation, the colors ranged in purity from 40 A. for red, to 15 A. for blue. This is pure enough for studies of animal color vision, since the range is well below the normal human limen.

In actual experimental work on macaque and cebus monkeys the two

[2]Below is an itemized account of the costs of building the two monochromators. All of the labor except welding two pieces of channel iron to form an angle at the prism, and milling one edge for the sliding lens supports, was done by the author, and his labor is not included in the expense. The prices quoted on the prisms and lenses include the import duty, with the exchange rate as of June, 1933.

2 prisms, Steg and Reuter, Germany	$43.34
4 lenses, R. and J. Beck, England	21.36
6 electric lamps, G. E., T 17, clear	6.00
Materials(brass, mirrors, screws, etc.)	3.00
Labor (welding channel iron and milling edge)	10.00
Transformer	7.00
Total	$90.70

monochromators have given approximately thirty hours of trouble-free service, and have proven to be quite suitable. The system of experimentation is as follows: The two instruments are placed side by side above two white-topped food boxes mounted on a slide. Behind the slit on each instrument is a mirror for reflecting the light toward these boxes. Each mirror is so mounted that a spot of colored light which it projects downward can be shifted from one food container to the other by a simple rotation of the mirror. The experimenter's position is on a chair underneath the instruments, behind the slide with the food boxes, and facing the monkey cage. The mirrors for shifting the colors are controlled by strings within easy reach. While making adjustments and placing the food reward, the experimenter is hidden from the monkey's view by a sliding shield. No one-way screen cutting off secondary cues from the experimenter is necessary, since, with the exception of the color patches, the experiment is conducted in total darkness. After the monochromators have been adjusted for the two desired colors and brightnesses, the experimenter can remain comfortably seated and give trials about as rapidly as the monkey's motivation permits.

IV. Summary

By using a single-straight-filament lamp as a substitute for a source slit, the author has constructed a color-discrimination apparatus especially adapted for experimentation on subhuman primates. It is simpler and considerably less expensive than previous instruments using spectral colors. Also, with two independent, identical instruments, the labor of experimentation is greatly reduced, and tests for color limens can be made by the simultaneous-presentation method.

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BOOKS

NORMAN L. MUNN. *A Laboratory Manual in General Experimental Psychology.* Pittsburgh: Univ. Pittsburgh Book Store, 1934. Pp. 114+ supplementary question pages, lettered *a*.

One of the characteristics of the manual under discussion which strikes the reviewer as a particular advantage is its scope. Although it seems unlikely that many instructors will find adequate time at their disposal for all 65 of the experiments offered, the choice is so wide that anyone would find it possible to organize on its basis a course consisting of representative experiments. Almost invariably a laboratory course is worked out to fit the apparatus available in a particular institution, and as Dr. Munn points out in his preface, that was the case here; many instructors will find, therefore, that their pet pieces of apparatus are not required, whereas they need others not at their disposal. The reviewer, however, has the impression that almost any laboratory either has or could construct the materials requisite for the majority of the experiments. Some of the experiments are historic landmarks in the field of experimental psychology; as might be expected, this is particularly true of the 18 experiments in the section on the sensory processes. Others, on the contrary, are new; these are usually recent research studies so adapted and modified as to be suitable for laboratory use.

The kind of material covered is most interesting. For example, it is something of an innovation (and a welcome one) to find a section of four experiments on social phenomena, and one of five on animal behavior, in a manual on experimental psychology. It is, perhaps, significant of a modern trend in this field! Although not every instructor will have either the facilities or the inclination to require these experiments of his students, for many it will suggest interesting possibilities in the direction of unusual and worthwhile procedures.

Dr. Munn's subject-predicate-object sentence structure is colorless and monotonous, but concise and unambiguous. We would, however, seriously criticize the brevity of his descriptions of apparatus and procedure. In many cases a reference is available where a more detailed description can be found, and in some cases a diagram of the apparatus is given, but this is not invariably the case. The manual could not be recommended for use by an instructor without excellent experimental training.

The author says that the selection of the experiments is eclectic, but his behavioristic tendencies are everywhere visible. For example, much emphasis is placed on learning, upon the nature of sensory discrimination, upon the nature of the physical stimulus, and upon the nature of the receptors, nerve impulses, and central mechanisms. There will be plenty of people to

say that the two latter topics are not the subject-matter for a laboratory manual in *psychology*. There is practically no demand upon the student for introspective (or even verbal!) reports, even in those experiments in which such a procedure might be used; with the exception of one experiment upon the types and relative vividness of imagery, the emphasis is consistently placed upon objectively obtainable results. Whether this is a constructive or destructive criticism, of course, will rest with the reader! Aside from finding our pet experiments upon the affective values of colors and odors in a section labelled Experimental Aesthetics, we have little to criticize either with respect to the selection of the experiments or to their systematic arrangement. In any case, a sufficient variety is offered, so that the instructor can choose such as will best suit both his laboratory facilities and his theoretical predelictions. To the reviewer, the Behaviorismus is obvious, but not objectionable.

The references for the most part are to books or readily accessible journals. Undoubtedly some readers will criticize the choice of journal articles, which in many cases are to experiments on highly specialized topics, not directly related to those discussed in the manual. They do, however, cover a wide range of topics, so that that *rara avis,* the student who wants to do more than he is required to do, will find much to keep him busy.

From the mechanical point of view the manual has much to recommend it. The experiments are printed on one side of the page only, leaving room for notes, results, etc. After each experiment there is a supplementary page with three or four significant questions, to be answered either from the results obtained from the experiment itself or from the suggested readings. These pages are perforated and can be torn out to hand in; a uniformity of style should thereby be induced in the written reports of the experiments, which will be much appreciated by instructors with large classes.

It occurs to the reviewer that it would help many instructors to know approximately how long the experiments require, i.e., were they intended for a two- or a three-hour laboratory period, and do they require all of a period, more than one period, etc.

Although the manual was frankly worked out to fit the material available in one laboratory, the selection of the experiments is such that it will be usable by many others. The reviewer is impressed with the catholicity of the field of *general* experimental psychology, as Dr. Munn sees it, but would not be surprised to find others who would object to such a broad definition.

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The appearance in rapid succession of several treatises on the historical development of psychology would seem to indicate that psychologists are rapidly becoming historically minded. It is now suggested that even the elementary student, "to gain a proper sense of value and a perspective of the present-day opinions in psychology," should have some contact with historical antecedents in the field.

Hulin aims to present to the beginning student a brief, elementary account of the development of psychology. After a lengthy discussion of the philosophical periods, important aspects of modern scientific psychology and certain special lines of interest are listed. The book ends with a brief discussion of systems.

There are possibly two lines of development prior to the immediate antecedents (physiology of early 19th century, and evolution) of experimental psychology which justify a survey of this early period by the elementary student: (1) The origin of the mind-body distinction, the development and definition of this dichotomy up to the point of complete separation of mind and body by Descartes, and the subsequent effects of this separation. (2) The origin and development of the differentiation between philosophy and science, which did not reach a sharp distinction until the 17th century. Although the first of these two phases of historical development is discussed specifically and the second incidentally, neither trend is adequately elucidated for either elementary or advanced students.

In considering the modern period and experimental psychology, names and events are cited without organizing these into coherent patterns of psychological trends. In fact, it appears that too many events are included. In an elementary text especially, one feels that only the more important works and authors should be cited. Included, however, are contributions of such men as: Gorgias, Pyrrho, Cicero, St. Anselm, Machiavelli, Luther, Geulincx, Boll, and Kühne.

The successful selection and organization of materials for any elementary text is no easy task. This first attempt with the history of psychology will receive a sympathetic welcome by many teachers in the field.

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